WESTERN EUROPEAN RADIOCARBON DATES AND HOLOCENE MARINE CHANGES, WITH SPECIAL REFERENCE TO CONCEPTS INVOLVING SCOTTISH ARCHAEOLOGICAL MATERIAL

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Declaration

I declare that as required by regulation 2.4.15,

a) this thesis has been composed by myself

b) the work is my own.

Signed,

(Ian A. Morrison)

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ABSTRACT

The concept of a single "25 foot raised beach" has dominated opinion on the relationship between Scottish archaeological material and sea level change for most of the present century. Recent work has however demonstrated that Holocene coastal changes were much more complicated than this concept suggested.

These complexities have not yet been fully resolved, either in Scotland or elsewhere. Doubt exists as to how far sequences of changes observed in one area are likely also to be represented on other stretches of coastline. At present, Scottish geomorphological and archaeological evidence does not in itself appear adequate for a reliable evaluation of this.

The seaboard of Western Europe between north Norway and Biscay contains a substantially wider range of Holocene land movement regimes and coastal environments than is represented in Scotland, and thus offers a basis for assessing the relative importance of ocean level variations and more local factors.

The evaluation of this type of interplay and the isolation of the eustatic(ocean wide) component has long been a matter of controversy in the literature. During the past decade, however, almost a thousand radiocarbon dates relevant to Holocene coastal changes have become available in Western Europe. These permitted the development of a new type of approach to the problem, based on a detailed analysis of the timing of episodes of transgression and regression.

From this it became apparent that despite the diversity of conditions on the European seaboard, the ubiquitous influence of ocean level variations had dominated the timing of shoreline changes throughout the Holocene. The only major exception was the Baltic, when cut off from the ocean during

periods such as the "Ancylus Lake" stage, but it proved possible to define these phases closely in terms of C^{14} chronology.

None of the published Holocene eustatic curves appears to be based on more than about 10% of the number of radiocarbon dates included in the present survey. Accordingly, a new curve taking these dates into account was derived.

The eustatic and other data from the survey were then compared with the Scottish evidence, using detailed information now available for the Forth-Tay area as a control. It was found that the Scottish data could be interpreted in a way consistent with the results from the remainder of Western Europe. A model of relative sea level change was constructed, and discussed in terms of the available archaeological material.

It was concluded that although necessarily provisional, this model appeared to offer a hypothesis for future investigation that seemed potentially more profitable than that provided by the "25 foot raised beach" concept.

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P R E F A C E

Almost two and a half millennia ago, Plato commented that men tend to establish themselves on the shore of the sea, like frogs on the edge of a pond.

In Scotland, as in Greece, it would seem that the coastlands have always formed an important element in the settlement pattern. Certainly both geographers and archaeologists have long emphasised that in "Highland Britain" the juxtaposition of contrasting resources at the coast has been an enduring factor in the human geography of prehistoric as well as of historic times (e.g. Fox 1932, Piggott 1958, Evans 1964, Walton 1966).

Dr Glyn Daniel (1963) has drawn attention to the importance of "geographical prehistory" as an element in models of the past now emerging. However, before an adequate human geography of the coastlands through prehistoric time may be attempted, it is necessary to have knowledge of the changes in the relative levels of land and sea that have occurred within the period for which archaeological material is available (in Scotland, this is the postglacial period, see Chapter 1). The aim of the present work is accordingly to contribute towards the elucidation of Holocene marine changes, to help create a basis for future studies of their implications for the prehistoric inhabitants of the coastlands.

It will be shown in Chapter 1 that the concept of the "25 foot raised beach", once regarded as offering a secure basis for dating Scottish archaeological material, can no longer be considered reliable. After examining the historical development of opinion on the archaeology of Scottish shorelines in Chapter 2, in Chapters 3 and 4 the nature of the available archaeological and non-archaeological evidence is considered, and its potential for evolving a more profitable hypothesis is examined.

It is concluded that in order to develop a dependable chronology of the changes, free from the circular arguments common in the past, initially it would be preferable to segregate these two types of evidence, and to attempt to develop a new model of marine change primarily on grounds independent of archaeological arguments concerning chronology. In particular, it is suggested (Chapter 4) that at the present state of knowledge it would be a useful contribution, not only to the elucidation of specific Scottish problems but also to the general understanding of the nature of Holocene marine change, if the relative importance of local and ocean-wide ("eustatic") factors in the timing of transgressions and regressions could be evaluated.

Almost a thousand radiocarbon determinations referring to Holocene changes of relative sea level are now available for the coast of western Europe. Between Biscay and the Arctic, this seaboard embraces a considerably wider range of coastal environments and land movement regimes than that represented in Scotland. The breadth of this spectrum offers a considerable measure of control for assessing how far coastal changes are likely to be synchronous in different localities within the more restricted range of conditions characteristic of Scotland.

Accordingly, in Chapters 5 to 11 a detailed survey is made of the Western European radiocarbon determinations. When the comparative stratigraphy, chronology and geographical distribution of the changes they refer to has been examined, the roles of eustatic and local factors are assessed. Then, in Chapter 12, an attempt is made to construct a curve of ocean level variation consistent with the results of this analysis.

In Chapters 13 to 15, Scottish data are compared with the pattern of events suggested by the more widely based study of the preceding chapters. This is done firstly with reference to recent intensive geomorphological and palynological studies in the Forth-Tay area (Chapter 13). Then in Chapter 14 sea level changes in the country as a whole are discussed, before the compatibility of the results with the evidence of available Scottish archaeological material is reviewed in Chapter 15.

The conclusions from the study as a whole are summarised in Chapter 16.

An interdisciplinary investigation of this nature, involving the discussion of numerous individual radiocarbon determinations and archaeological sites, is necessarily somewhat lengthy. For ease of reference, it may be noted that the principal types of material are located as follows:

i) Western European radiocarbon dates referring to marine changes (excepting Scotland): Chapters 5 to 11, with all dates listed by laboratories in the main Appendix.

ii) Non-archaeological aspects of relative sea level change in Scotland: Chapters 1, 13, 14 (with Scottish radiocarbon dates).

iii) Scottish archaeological concepts and material: Chapters 2, 3, 15.

C H A P T E R 1

INTRODUCTION

In principle, the study of the former relationships between land and sea levels offerm a promising approach to the problem of developing chronological frameworks for the prehistoric period. Changes in the level of the oceans are essentially synchronous on a world wide scale, and although changes in the level of land masses vary in nature and amount from place to place, they frequently follow orderly patterns over great areas.

Events characterised by as widespread geographical distributions as these present considerable potential to the archaeologist. Besides allowing checks to be made on archaeological dates proposed on the basis of other kinds of chronological argument, the stratigraphic or geomorphological correlation of archaeological material with episodes of sea level change can sometimes offer a means of giving a chronological context to sites or material that can not readily be dated otherwise. For instance, flint artifacts of problematic typology not themselves susceptible to direct chronometric assay can sometimes be placed relative to a datable sequence of coastal changes.

In practice, for many years it seemed as if the raised beaches of Scotland offered a straightforward and reliable guide for dating the earlier Scottish archaeological material. As will be shown in Chapter 2, the interplay between geological and archaeological ideas forms a recurring theme in the Scottish literature. It can be traced back through the better part of three centuries, and it is no less evident in recent decades.

For example, as eminent a prehistorian as Professor V.G. Childe stated in "Scotland before the Scots" (1946, p3) that the age of the early cultures of hunters and gatherers was "guaranteed by geology; for they lived on the shore of the sea, when its waters stood some twenty-five feet higher than today." A.D. Lacaille, in "The Stone Age in Scotland" (1954, p55) describes what he calls "The Early Post Glacial raised beach" as "of supreme importance to Scottish geology and archaeology", and makes the statement that "the first unambiguous indications of settlement ... are all associated with Early Post Glacial marine deposits" (p124). In what is still one of the most recent books covering the full span of Scottish prehistory (Piggott, ed., 1962 p16), Professor R.J.C. Atkinson wrote of cultures of Mesolithic aspect that "the principal evidence upon which these signs of early accupation have been dated is their relationship to the 'raised beach' deposits". Some very recent work on Scottish stone artifacts also places considerable emphasis on concepts involving shoreline change (e.g. Mercer 1968, publ. 1970).

By the beginning of the present century (viz. D.E. Smith 1965; & Chapter 2, below), it indeed seemed as if the pattern of the Scottish former shorelines was well understood. The literature of the first half of the 20th century is dominated by the concepts of "100 foot", "50 foot" and "25 foot" raised beaches". Archaeological material has been almost invariably considered only in terms of the last of these, since only the "25 foot raised beach" was considered to be post glacial. (With very little dissent, e.g. F. Smith 1909, Mann 1936, it has been accepted that no remains of man are known from the glacial period in Scotland.)

The dating of the "25 foot raised beach" has however been a matter of some dispute. It has been attributed to the Palaeolithic, Mesolithic, Neolithic, Bronze Age and Roman periods (see below, Chapter 2). For most of the last three decades it seems to have been widely accepted as Mesolothic, but particularly in recent years some archaeologists have concluded that it was somewhat later than many have tended to think (e.g. Coles 1964; Scott 1966). It has also been suggested (among archaeologists, notably by Mercer, op. cit.)

that apparent conflicts in the evidence might be explained if the highest postglacial transgression was of different ages in different parts of the country.

Despite the diversity of these views on dating, the concept of relative sea level change in Scotland used by prehistorians has tended to remain an essentially uncomplicated one: that of a single abandoned shoreline from within the period for which archaeological evidence survives. On several grounds it now seems clear that this concept is a misleading one.

Even in the 19th century, some fieldworkers (notably Yule, 1866, in the east and Dougall, 1867, on the west) had observed that there appeared to be evidence of several sealevels in the Scottish postglacial marine deposits. By 1906, as important a figure as T.F. Jamieson (whose contribution to the theory of isostatic shoreline displacement is still valued today) was denouncing concepts such as the "100 foot" and "25 foot raised beaches" as mere "articles of faith" rather than observed facts. From time to time, others reported very complex arrays of postglacial coastal landforms, from localities as varied as Irvine (on the Forth of Clyde (J. Smith 1896)), the Moray Firth (Ogilvie 1923) and West Jura (Ting 1936). By the nineteen thirties, the Geological Survey officially accepted that more than one postglacial shoreline was apparent in the north of Scotland (Phemister 1936), but although Phemister's "15 foot raised beach" was subsequently accepted by some as having a widespread distribution in Scotland (e.g. Donner 1959, 1963), until very recently this complication of the "25 foot raised beach" concept was not considered a sufficient reason for any overall reassessment of that concept by geologists. Andrews' recent statement that "research in Great Britain foundered on the so-called '100, 50 and 25 ft.' beaches" (1970 p11) is forcible but perhaps not unfair.

As D.E. Smith (1965) and J.B. Sissons (1967) have pointed out, the concept of "100 foot", "50 foot" and "25 foot raised beaches" persisted for so long essentially because the need for accurate, detailed measurements of the altitudes of raised shorelines was not appreciated. One contributory factor in this situation would appear to be the fact that the contoured editions of Ordnance Survey maps first became widely available during the late 19th century, at the time when the conception of the three-dimensional pattern of the raised beaches was evolving. It is undeniable that there are many former shoreline features of postglacial date around the Scottish coasts, that coincide approximately with the 25 foot O.D. contour of the Ordnance Survey maps. Similarly, the 100 foot O.D. contour crosses many late-glacial shoreline features. Moving from place to place with a map it is thus tempting to assume that features on the same contour belong to the same shoreline.

As fieldwork progressed in the latter part of the 19th century, and opening decades of this century, it nevertheless became generally agreed that the most visible postglacial shoreline features were not in fact everywhere 25 feet above the present sea level. They seemed higher in the central areas of Scotland, perhaps reaching almost to 50 foot O.D., and appeared to slope outwards, towards or indeed to below sealevel on the periphery. Measurements of actual height were few and far between however, and the old "25 foot" name was maintained.

J.B. Sissons has estimated that outside the Forth/Tay area, fewer than two hundred measurements of the heights of Scottish raised shorelines have been made by accurate instrumental levelling from the Ordnance Survey beachmarks. This total includes late-glacial as well as postglacial features (Sissons 1967, p 167).

Other measurements have certainly been made, for instance by aneroid barometer, and often using not Ordnance Survey benchmarks, but high-water mark or the upper limit of seaweed or barnacles to provide a datum. Such methods however, introduce inaccuracies that prevent the reliable differentiation of former shorelines that are separated by small vertical height intervals. This is illustrated by Figure 1.1 (after Sissons, 1967 p167), which shows the results of an experiment carried out near the Lothian Tyne, comparing aneroid measurements taken in favourable atmospheric conditions by J.B. Sissons, with Dumpy Level measurements taken on a closed traverse by Sissons and the present writer.

This diagram illustrates the possibility of miscorrelating shoreline measurements, even within a distance of half a mile if an imprecise instrument such as an aneroid has been used. The problem of resolving features with a close vertical separation is further exacerbated when, as has often been the case, approximate measurements have been spaced miles or even tens of miles apart, so that no adequate quantitative allowance can be made for local variations of shoreline height due to such factors as differences in exposure or in conditions of deposition.

Considerations such as these led to a major investigation of the Forth and Tay valleys, together with the intervening coast of East Fife (henceforth referred to as the Forth-Tay area). Working independently, but with standardised techniques so that results would be directly comparable, R.A. Cullingford, D.E. Smith and J.B. Sissons mapped the former shorelines and related landforms of the Forth-Tay area at large scale (1:10,060) on Ordnance Survey base maps. The heights of all shoreline features were then established in detail, by accurate instrumental levelling using closed traverses on Ordnance Survey benchmarks. The height measurements were taken at 60 to 80 yard intervals, continuously along the length of all identifiable shorelines. In addition, large numbers of commercial bore-hole logs were analysed, and several hundred hand bores were sunk in order to establish the location and height of buried shorelines and related features. References to publications describing this investigation and identifying other workers who have contributed towards it, are given in Chapter 13 where aspects of the results are discussed in detail.

In the course of this investigation the three-dimensional pattern of the late and postglacial shorelines of the Forth-Tay area was established in terms of well over ten thousand accurately measured heights, ie. over fifty times the number of measurements of equivalent accuracy that had previously been available for the whole of Scotland, and certainly ten times the total number of measured shoreline heights of all kinds, even including unsatisfactory aneroid readings.

The Forth-Tay results show that there are not one or two postglacial shorelines (e.g. the "25 foot" and "15 foot" raised beaches) in that area, but instead a complex array of both exposed and buried Holocene features. There are at least three raised but buried shorelines (one apparently dates from the retreat of the ice at the very beginning of the Holocene, but the two others seem fully postglacial). Traces of peat in some bores suggest that these three may not represent the full story of the part of the sequence that is now beneath later marine deposits. There are at least four unburied raised shorelines that date from the Holocene, but, similarly, it is by no means certain that these represent the complete unburied sequence. There are, for example, several clearly visible additional terraces on the north side of the Tay estuary. As yet it is not clear whether these are dominantly marine or fluvial in origin.

It is thus evident that even on the most conservative interpretation of the new quantitative data, Holocene changes of relative sealevel in this area would appear to have been much more complex than was recognised in the traditional view of Scottish raised shorelines.

Although the results of the Forth-Tay investigations thus contrast with much that has been published regarding Scottish shorelines in the last half century, the greater level of complexity indicated by the Forth-Tay measurements conforms well to modern opinion on the nature of relative sea changes elsewhere on the western seaboard of Europe during the Holocene. General accounts illustrating this include: Donner (1965), Finland; Zenkovich et al. (1960), U.S.S.R; Lundqvist (1965), Sweden; Feyling-Hanssen (1964), Norway; Sigurd Hansen (1965), Denmark; Müller (1962), Germany; de Jong (1967), Netherlands; Alimen (1967), France. More detailed papers are indicated in the bibliography accompanying the corpus of European C^{14} dates in the Appendix. In comparison to the characteristically complex sequence of changes reported from widely varying conditions of coastal environment and land movement regime elsewhere in Europe, the simplicity of the Scottish "25 foot raised beach" concept seems anomalous.

This is discussed further in a later chapter, but even a superficial perusal of the European evidence suggests that it would be unwise to disregard the possibility that other parts of the Scottish coastline may have undergone changes at least as complex as those indicated by the detailed measurements made in the Forth-Tay area. The very limited amount of reliable quantitative data available concerning Scottish raised shorelines outside that area, however, makes it difficult to evaluate this immediately. This is illustrated by Figures 1.2 & 1.3, which refer to the work of S.B. McCann (1966).

Some seventy of the two hundred or so shoreline heights accurately levelled in terms of Ordnance Survey Datum that are available from outside the Forth-Tay area were obtained by McCann. Each of his measurements was made at a different site, at one of seventy separate locations between Ullapool and

Oban, and including Skye and Mull as well as the mainland. He computed a simple plane to characterise the overall three-dimensional pattern of this data, and Fig.1.2 (after his Fig.4A 1966 p97) shows the heights of all 70 sites projected onto a base line drawn in the direction of maximum tilt of that plane. Onethe assumption that all the heights referred to a single former shoreline, he calculated the regression line shown in Figure 1.2, and concluded "it is possible to recognise throughout the area a single raised shoreline, the 25-foot raised beach of the literature." (op.cit. p99)

His sites range from some with full exposure to the winter gales of the Minch, to others landlocked in the shelter of Loch Eil (inland of Fort William) With differences of exposure of this order, erosional features of the type he measured would certainly show a considerable variety of relationships to their contemporary sea level (viz Chapter 4). This clearly contributes to the scatter of the height points on his graph.

McCann, however, notes that he encountered not one but several postglacial shoreline levels. He states that his measurements refer only to the most marked feature at each.site. As noted above, he assumes that this could be accepted as representing the same shoreline throughout the area, although he comments (op.cit. p97) that at no site was an individual shoreline fragment in fact followed for more than two hundred yards in the course of measurement. In as extensive an area as he studied, it would seem to be difficult to be confident on this basis that the feature measured at the individual sites all referred to the same ancient water plane.

The considerable differences in exposure of the sites, and in the materials in which the measured features were cut, would seem to make it unlikely that the most marked erosional feature surviving at each place would necessarily be the same age as elsewhere. It would seem not unreasonable to expect that in the more exposed sites, earlier features would be more likely to be cut away and destroyed by later erosion than in the more sheltered localities. The possibility of older features surviving better in the more sheltered localities would appear to be considerably increased by the way that the sheltered sites of Lochs Linnhe and Eil appear to lie in areas subject to considerably more glacio-isostatic rebound, so that earlier features there would tend to be removed from the reach of later wave attack more effectively than at the less isostatically uplifted exposed sites on the Minch.

Figure 1.3 shows the major postglacial shorelines visible in the Forth-Tay area, redrawn after Sissons, Cullingford and Smith (1966 p11), and superimposed on McCann's height data at the same scale. It must be emphasised that, because of the different nature of the features measured, there can be no direct equivalence between the two sets of data. Nevertheless, the figure serves to illustrate that the scatter of heighted points that McCann attributes to a single "25 foot raised beach" occupies a range as large as that in which at least 4 visible Holocene shorelines (and at least 3 buried ones) may be distinguished elsewhere within Scotland.

McCann's data were chosen for this example for two reasons. Firstly because they represent fully a third of the accurately levelled heights available for Scottish raised shorelines outside the Forth-Tay area, and secondly because his is the most recent substantial study in which an author has concluded that measurements supported the concept of the "25 foot raised beach". On the basis of the preceding paragraphs, it is suggested that McCann's data are not sufficiently detailed to sustain his conclusion. In view of the variables involved, and the fact that shorelines were not traced continuously by closely spaced measurements (as in the Forth-Tay area), but only sampled at widely dispersed and diverse sites, it is considered that the approach used was intrinsically incapable of resolving as complex shoreline variations as those which must now be considered possible, indeed probable, in the light of the Forth-Tay results and the European evidence in general.

In recent years, Donner, as well as McCann, has been notable for supporting the traditional concept of the Scottish former shorelines. In papers published in 1959 and 1963, he considered that he identified both the "25 foot raised beach" and "15 foot raised beach" in first northern and central, then southern Scotland. His data, however, included only 10 heights (all in Argyll) levelled in terms of Ordnance Survey Benchmarks. Three other heights were levelled in terms of the barnacle line, but he notes that this varied in height from place to place. All other heights were obtained only by aneroid barometer.

In his latest paper on Scotland, Donner (1970) has clearly reconsidered his position. Discussing "an often well developed terrace ... known under the name of the 25-foot beach" he notes confusion can occur between "raised beaches at about the same altitude, especially when comparisons between widely separated areas are made" (p34), and goes on to conclude (p35) that "to avoid confusion with other shorelines the term 25-foot beach has been abandoned in the most recent investigations ... As several shorelines, and not only one, have been separated below the Main Postglacial Shoreline, the term 15-foot beach has also been abandoned" and notes in addition "Young Flandrian transgressions, such as are recorded further south in Britain, may also have occurred in Scotland".

The most extensive attempts yet made in Scotland to explore the newly acknowledged complexities of the Holocene changes have been those of the Forth-Tay team on the east coast, and of Stephens and Synge of the west coast (e.g. Stephens 1968; Synge & Stephens 1966). Unfortunately, while there is agreement that the changes are undoubtedly complex, the principles, methods and results of the two enquiries differ substantially and a dispute has arisen (e.g. Sissons 1967 - Comments on Synge and Stephens 1966; Synge and Stephens 1967 - Reply to J.B. Sissons's Comments).

The specific results of the two enquiries will be discussed in some detail in later chapters. Basically, Stephens and Synge propose five postglacial shorelines, but Sissons criticises both their interpretation of shoreline features and their techniques of measurement. The present writer concurs with the former criticism and also with several of the points made regarding measurement accuracy, though Sissons's scepticism of the figures quoted by Stephens and Synge for the accuracy of their zeroed hand-level seems unjustified (viz Debenham 1954). Even if the accuracy of individual measurements is somewhat higher than Sissons believes, however, the scattering of the limited number of measured sites (ca 36) from Loch Etive to Dublin certainly would appear to render the data more liable to miscorrelation than in the case of the Forth-Tay study, where it will be recalled that the workers attempted to measure all surviving shoreline fragments with heights spaced at 60-80 yard intervals throughout their lengths.

Synge and Stephens criticise Sissons for disregarding the methodological contributions of early Scandinavian workers, such as Tanner (1931) whose form of "relation diagram" they use in the correlation of their own shoreline heights. However, as Andersen (1965) and Mörner (1969) amongst others point out in the recent Norwegian and Swedish literature, both fieldwork and theoretical studies (e.g. by Marthinussen 1960, & Holtedahl 1953, inter alia) have established that "relation diagrams" are "definitely not as accurate" (Andersen op.cit. p127) as "equidistant" shoreline diagrams of the type used by the Forth-Tay workers. Marthinussen has gone as far as to state that "the relation diagram is incapable of giving correct results" (1960 p420).

In light of this, and the fact that Synge and Stephens's specific criticisms of the Forth-Tay study refer to the late-glacial and not the postglacial period, it might be considered that the pattern of Holocene change established by the Forth-Tay investigation could be accepted as the model for Scotland.

The Forth-Tay area certainly offers by far the most precise and complete set of data so far available on Holocene shoreline changes for any large part of Scotland. Results from any single area are however of limited value, even when they can be thoroughly documented in quantitative terms. It is difficult to evaluate from the Forth-Tay evidence in itself how far the details of the patterns represented there are likely to apply elsewhere, and how far they may be the product of essentially local factors. Furthermore, as was stated above, there are some suggestions in the Forth-Tay area of other changes in marine influence, imperfectly represented there and additional to the more clearly apparent parts of the sequence. This introduces the possibility that episodes that have left little or no record in the Forth-Tay area might well be represented by prominent features elsewhere in Scotland, where different combinations of land uplift and coastal environment have prevailed. The limitations of the Forth-Tay area as a model for Scotland are exacerbated by the fact that many of even the best established changes in marine influences there are as yet undated, while some of the radiocarbon dates that are available are difficult to evaluate geologically.

<u>Conclusion</u>

It would thus seem that a very real problem exists for archaeologists and others interested in the relationships between Scottish antiquities and marine changes.

As indicated above, in recent years those concerned with major studies of Scottish postglacial marine features have come to agree on their complexity. (It will be recalled that although McCann favoured the existence of the "25 foot raised beach", he also acknowledged the presence in his field area of multiple postglacial shorelines.) Although controversies certainly exist regarding the correlation of observations from different sites, the field studies of amongst others, Cullingford, Sissons and D.E. Smith in the south-east, McCann, Stephens and Synge in the southern half of the west coast, and Godard in the north-west, have all unambiguously confirmed that flights of multiple postglacial shorelevels occur in Scotland. As Godard has put it (1965 p625) "Les apparences de simplicité qui caractérisent les grands fragments bien conservés de ce 'rivage soulevé inférieur' (la "25-foot beach" des Britanniques) sont en réalité assez trompeuses".

It would thus seem injudicious to place much weight on arguments involving the "25 foot raised beach", in attempting to develop chronological frameworks for the use of archaeologists or geographers interested in the prehistoric period.

Discussions at the Scottish Archaeological Forum, held in Edinburgh in 1969, suggested to the writer that the very simplicity of the early concept had been a major element in the value attributed to it in the archaeological literature. It appeared to offer a notably straightforward basis for comparing the ages of archaeological material in widely separated parts of Scotland.

For all its convenience, however, the traditional concept offered only a single dating horizon for the postglacial period. The complex sequence of shoreline changes now becoming apparent seems to extend throughout the greater part of the Holocene. In theory at least, the very complexity of the changes and the length of the period they occupy would appear to offer greater rather than less potential for detailed chronological or environmental studies.

In practice, however, the realisation of this potential appears to present considerable difficulties. The chronological correlations proposed in the past on the basis of the "25 foot raised beach" concept were made essentially on morphological grounds, with (as indicated above) very little actual measurement of the features involved. If Cullingford, Sissons and D.E. Smith are correct, the Holocene marine changes affecting Scotland have been sufficiently complex for it to be necessary to use accurate measurements spaced continuously along shoreline remnants at very close intervals (perhaps 60-80 yards) in order to resolve these changes morphologically.

The Scottish archaeological sites reported to relate to marine changes are however dispersed around virtually the whole coastline of the country (see Chapter 3). It seems most unlikely that large sectors of the coastline of interest to archaeologists will be covered by geomorphological measurements of the Forth-Tay standard in the forseeable future.

Furthermore, it will be recalled that even in the Forth-Tay area, where conditions are particularly favourable for both registering and preserving evidence of changes in marine influence, there are suggestions that the detailed programme of investigations has not revealed the full story of the changes affecting that area. Many of the most interesting archaeological sites are located in other areas, such as that studied by McCann, where conditions are much less favourable for the analysis of the Holocene marine sequence and it is possible that because of variations in exposure and preservation of evidence even very detailed measurements might fail to elucidate the local details of that sequence on morphological grounds.

There would thus appear to be a greater necessity than has been realised in the past, not only for more comprehensive and accurate measurement of the landforms, but also for the wider use of other approaches, complementary to the morphologically based one, in the analysis of Holocene changes in marine influence.

At present, relatively few radiocarbon dates are available for marine changes in Scotland, and these are unevenly distributed both through time and in terms of geography. The same is true of relevant palynological studies. With the great majority of detailed and accurate morphological measurements confined to only one major area, and only a much smaller number of comparable measurements available elsewhere, considerable doubt thus remains as to how far events recognised in one part of the Scottish seaboard are likely also to have occurred elsewhere.

At the current state of knowledge, this makes it difficult to utilise information regarding marine changes in concepts concerning Scottish archaeological material. Before general progress can be made, it would seem necessary to assess to what extent the timing of changes in the relationship of land and sea is likely to reflect local and regional combinations of circumstances, and to what extent it is likely to bear an immediate relationship to variations in ocean level.

In practical terms, if local factors are altogether predominant, changes in relative sealevel are likely to be of minimal interest to those concerned with developing chronological frameworks for archaeological material. If, on the other hand, the timing of the complex changes now becoming apparent instead reflects primarily variations in ocean level, this would be of considerable interest and potential value, since these 'eustatic' changes are essentially synchronous everywhere.

It thus seems likely that the answer to this question will play a cardinal role in determining the future development of concepts relating marine change and Scottish prehistory. The central aims of the present study are, thus, firstly, to secure some clarification of this problem, and, secondly, to seek to develop a more profitable provisional hypothesis for future investigation in Scotland than that presented by the concept of the "25 foot raised beach".

Figure 1.1 Accuracy in shoreline height measurement - after Sissons 1967, Fig.71, p.167

Fig. 71. Measurement of two raised shorelines in East Lothian by aneroid (upper diagram) and accurate levelling (lower diagram).

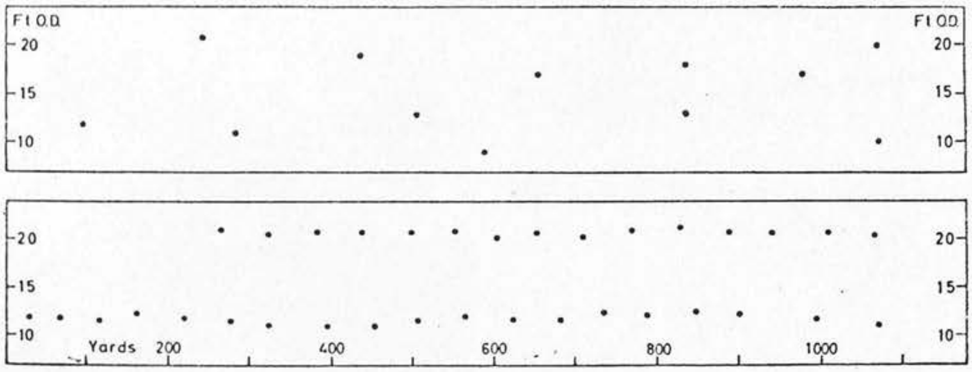


FIG. 71. Measurement of two raised shorelines in East Lothian by aneroid (upper diagram) and accurate levelling (lower diagram).

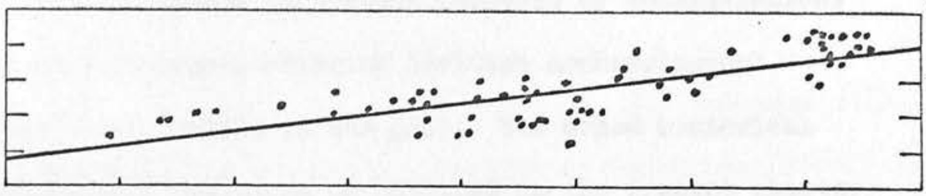
Figure 1.2 Interpretation of dispersed shoreline measurements by McCann - after McCann 1966, Fig.4a, p.97.

Figure 1.3 Comparison of 1.2 with results from the Forth-Tay area (as summarised in Sissons, Smith and Cullingford 1966, Fig.1, p.11. See 13.1)

1.2

1.3

1.2



1.3

CHAPTER 2

The historical development of concepts relating Scottish archaeological material to marine changes.

Introduction

The decisions leading to the specific type of approach adopted in the present research were influenced in several respects by considerations arising from the way in which concepts relating Scottish archaeological material to marine changes have evolved in the past. The broad historical evolution of these concepts will therefore be outlined in the present chapter, before the nature of the available archaeological and non-archaeological evidence is examined in specific terms in Chapters 3 and 4.

An historical review

In Scotland, former marine features often form an obvious element in the coastal landscape, and submerged and buried forest beds have a wide distribution. It is thus hardly surprising that here, as often elsewhere (e.g. Sollberger 1962, Graves 1969), speculations on changes of sea level within the human period appear to have been current from very early times and recur frequently in the body of oral tradition.

These "pre-scientific" attempts to give an account of Scotland's past have relevance in the present context, because it seems that the manner in which opinions on the relationship between archaeological material and sea-level change developed prior to 1900 has had an important bearing on the nature of the concepts current up to the present decade.

The roots of academic concepts concerning the archaeology of sea-level change developed early enough to fall within the period that Charlesworth (1957, p632) describes as that of the "vague surmises, crude generalisations grotesque assumptions and uncontrolled fantasies at the infancy of geology".

However, as will be shown below, in Scotland the emergence of fundamental modern concepts of geology took place relatively early in the 19th century, under the influence of such workers as Hutton, Playfair, Lyle and Agassiz. The very rapidity of these geological developments outstripped many antiquaries, and to the very threshold of the 20th century some of the latter failed to draw a clear line between what was in essence folklore and what would now be regarded as acceptable geological and archaeological evidence.

It will be suggested here that the contrast between the impressive progress of geological work on former shorelines, characteristic of Scotland during the late 19th century, and the weakness of contemporary archaeological speculation regarding marine changes, did much to contribute to the situation in which for the first sixty years of the 20th century archaeologists tended to be uncritical in their acceptance of what is now known to be an oversimplified geological model (cf. Chapter 1).

The pattern of 19th century opinions, and the nature of the substratum of oral tradition, will accordingly be examined before 20th century conceptual developments are discussed.

The nature of the Scottish oral traditions involving sea level change goes far to explain the persistence with which they were accepted. Thus, although some contain a generous amount of fantasy, many clearly arise from actual observation. In most cases genuine evidence of sea level change supported at least some of their elements. Some of the traditions are indeed fully acceptable today.

This is so, for instance, in the case of the rhyme referring to Lochar Moss by the Solway:

"First a wood, next a sea,
Now a moss, and ever will be..."

This couplet was already considered to be very old when Sir Daniel Wilson recorded it (1851, v.1., p44), yet its validity is demonstrated by modern studies, (e.g. Jardine 1964, Nichols 1967) which confirm that a forest bed is there overlain by marine clay that is in turn succeeded by a peat bog.

Often the only embellishment of an accurate interpretation of local evidence is that the account of the deduced event is put into the mouths of hypothetical witnesses. Fleming recognised this as early as 1830. After describing the submerged forest bed near Largo, he rejected the tradition amongst the townsfolk that their forebears actually witnessed the inundation and concluded that "it is probable that the tradition arose from an opinion expressed by some early and meritorious observer whose very name is unknown, and by whose influence it became enrolled in the legends of the neighbourhood."

From imagining witnesses it is but a short step to supplying a population for the lost landscape. One of the most persistent and widespread traditions of this kind in Scotland concerns a Princess of Harris, who hunted the stag and boar with her courtiers through a forest believed to extend out as far as St Kilda, from the now submerged peat beds off the shores of the Long Island. Martin Martin recorded this in the 17th century, and it is still current today (e.g. Beveridge 1913, Swire 1969).

These examples have been drawn from areas with buried or submerged forest beds, but they have counterparts involving most of the other types of evidence for sea level changes represented in Scotland. Thus, those who might doubt the tales of iron rings for tying up boats on, say, the inland rock on which Castle Huntly stands (Chambers 1848), or on the Clach nan Lunn on Gartmore hill (Cunningham-Grahame 1895), could have their attention drawn to manifestly valid evidences of changed sea levels in the neighbouring carse lands of Tay and Forth.

It will be noted that Cunningham-Grahame wrote as late as 1895.

In 1904 it was still necessary for Munro to publish a detailed criticism of Chambers's view, and others involving similar traditions.

The close juxtaposition of observable fact to attractive fiction in many of the traditions would seem to go far to explain the credulity of many archaeological writers on sealevel change even at the threshold of the 20th century. The weakness at that time of the archaeologist's position, relative to that of contemporary geologists, was emphasised by the disorganised nature of archaeological opinion on the dating of those coastal changes thought to involve man. Whereas, by then, Scottish geologists appeared to have achieved a simple and effective analysis of the three-dimensional pattern of the observed former shoreline features, amongst archaeologists ideas on sealevel change tended to remain confused and more obviously open to attack. Indeed, while geological work on former shorelines had advanced markedly in Scotland towards the end of the 19th century (see below), little equivalent advance on 18th and early 19th century thinking is apparent on the corresponding archaeological front.

Many antiquaries were content to follow those oral traditions which attributed changes af sea level to unspecified but remote periods of the human past, while simultaneously others favoured the Biblical Deluge, the time of the Roman occupation, or indeed even more recent times.

To protagonists of the Biblical hypothesis, the submerged forest beds were Noah's woods. Proof of the Deluge was seen in the finding of deeply buried canoes (see Chapter 3) or such things as an alleged camel, carved on a rockface on Canna (although, according to another tradition, it was on Harris that the Ark landed: Swire 1969).

Theories of the Biblical Deluge kind, which invoke catastrophic processes altogether beyond the realm of man's experience, had also figured prominently in early geological literature. However, Scottish workers such as

James Hutton (e.g. 1795), Playfair (e.g. 1822) and Lyell (e.g. 1830) played a leading role in swaying world geological opinion away from such "catastrophism". Despite the early date in Scotland of this move towards the accepted modern view that geological processes have generally been uniform in nature in both past and present, on the archaeological side naive "catastrophism" remained an element in opinion to the very end of the 19th century. Indeed, as late as 1901 and 1907, Pearson claimed all raised shorelines had originated within historic times, as the result of massive periodic shifts of ocean currents, although such views were by then long obsolete in terms of leading geological thought.

Attributions of Scottish sea level changes to the Roman period outnumber those invoking the Deluge. Indeed, throughout the 18th and 19th centuries, the Roman occupation was very widely favoured by those who sought to allot a specific period to a sea level change. This is hardly surprising. Not only were the Romans the first people of antiquity whose genuine material remains could be positively identified and dated in Scotland, but for long the tendency to regard sealevel change in terms of the Romans was further reinforced by the common assumption that any artifact showing evidence of technical sophistication could not be other than Roman. Sir Daniel Wilson drew attention to this as early as 1851, in discussing the bronze cauldron found in 1768 on the Forth carse "upon the surface of the clay, buried under the moss" and persistently referred to as "a Roman Camp Kettle". However, even towards the end of the 19th century, Marshall (e.g. 1880) was not alone in his casual attributions of finds of all kinds to the Romans.

Some of the most influential early writers on Scottish antiquities considered that there were major differences in the coasts in Roman times. This was stated as early as 1707 by Sir Robert Sibbald, in a standard work on

Stirlingshire that was last reprinted in 1892. He noted (p7) that "where the Carss ground is now, was then sea, which any may yet discern, for upon the digging of the Soil, some few Foots deep, there appear Beds of Shells, and the Water in the Channells, cut through that low ground is Brackish and Saltish ... Thus it was very uneasy for Agricola to penetrate into this Countrey ..." General William Roy, whose "Military Antiquities of the Romans in North Britain" (published posthumously in 1793) was also a work of longstanding influence, cautiously concurred (p153), and by the middle of the 19th century the idea of different sealevels in Roman times featured in such widely read works as Robert Stuart's "Caledonia Romana" (1845), and Sir Daniel Wilson's "Prehistoric Annals of Scotland" (1851). In some quarters, the view still carried considerable weight in the opening years of the 20th century. Thus, in 1903 Professor Edward Hull stated in a court of law on behalf of the British Museum that the Scottish beaches at about 25 ft OD were formed about the 4th century AD.

For a time, the concept of a high sea level in Scotland in Roman times gained support from the geologist Archibald Geikie. His 1861 paper was confined to the Firth of Forth, and he noted specifically that his deductions need not apply to the west coast of Scotland. However, in 1862 he extended his conclusion of post-Roman uplift first to the Firth of Clyde, and then to "the greater part of the British Isles". Sir Charles Lyell incorporated this view in the 3rd edition of his "Antiquity of Man", though he subsequently removed it from the 4th edition after sustained criticism had discredited Geikie's evidence (see below).

It is notable that Geikie's critics (e.g. Bryson 1861; Carruthers 1862; Yule 1866; Milne Home 1873) were essentially geologists rather than archaeologists. Although amongst antiquarians and archaeologists the view that sea level had been higher in Roman times continued to have some adherents to the very end of the 19th century, amongst geologists, on the other hand, as Geikie himself acknowledged, the view that there had been no change in the relative level of land and sea since the Antonine Wall was built had been widely accepted ever since James Smith of Jordanhill reported his investigations in 1838 and 1839. It will be suggested at a later stage that a similar lack of interdisciplinary contact between the archaeologists and geologists interested in Scottish sea level changes has persisted through much of the 20th century.

Archibald Geikie and the geologists who disputed with him on the Roman question attempted in general to draw their conclusions from direct observation of material evidence. They were not always successful (for example, according to Bryson, 1861, Geikie's "Roman" pottery included contemporary flower pots). Few, however, of the 19th century writers who favoured coastal changes at periods even more recent than that of the Roman occupation appear to have made any equivalent effort to secure firsthand evidence. The uncritical handling of evidence and general lack of rigour in argument compared most unfavourably with much of the purely geological work going on in Scotland at the same time.

Among the commonest types of evidence quoted for the archaeological date of sea level changes are accounts of the finding of iron objects, and particularly anchors buried far inland in elevated marine deposits. Almost none of the accounts even approach being firsthand. They are commonly of the form "I am assured that what was considered as the remains of an anchor were found some years ago in casting a drain ..." (Chambers 1848 p20), or "...pieces of broken anchor have been found here in the memory of people yet alive" (Nimmo 1777 p73). In the few cases where the pedigree of the evidence is traced in greater detail, the results do not inspire confidence. Thus, while one of the most circumstantial seeming accounts of an anchor is that given in the New Statistical Account of Perthshire (1845 p378), its source is stated as follows:- "The writer has conversed with a man, who told him that he recollects distinctly of hearing his father state that, at a period of about forty years ago ..."

In most cases, the alleged finds were pure hearsay, few indeed having actually survived to the writer's day. Even those finds whose physical existence and source could be established were frequently capable of explanation by less extravagant hypotheses than those envisaged in the literature. Robert Munro (1904) illustrated this with the case of an iron boathook head found at Inchmichael in the carse of Tay, 8 feet down, 20 feet above the level of the present high tides, and about a mile inland. This had been the subject of much speculation since its discovery in 1837, but he draws attention to the use of deep drainage cuts in the carse, noting that they become infilled in time, and that this could easily account for the deep burial of a boat hook head perhaps lost while poling a rowing boat up such a cut. He concludes with some asperity "... but surely it is unnecessary to discuss the possible ways in which such a portable object ... might have got strayed. The suggestion that it was lost by a sporting sailor on a wild-boar hunt is as feasible an explanation as that it was dropt from a sailing-vessel while the Carse lands were still submerged". (1904 p256)

In the same paper he effectively demolished not only views involving other recent iron objects, but also those still relating Scottish shoreline changes essentially to the Roman period.

It is notable that it was not until 1904 that so systematic an attack was made on the unsatisfactory nature of Scottish archaeological speculations regarding the date of marine changes. The importance of the threshold of the 20th century in the overall development of thought on the archaeology of these changes is also emphasised by the fact that it was not until then that archaeological sites bearing on the problem were excavated and reported to standards approaching those of modern practice.

Excavators including Mapleton (1873) and Symington Grieve (1882) had begun to show the way, but in many respects it was not until the work of Gray at Campbeltown, (1894), and that of Anderson's interdisciplinary team at Oban, (1895), that the 19th century emergence of the modern discipline of archaeology from traditional antiquarianism began to impinge on the problem of dating Scottish sea level changes.

Up to the beginning of the present century, the dating of Scotland's former sea levels thus tended to lie in a cul-de-sac off the main line of archaeological development. The majority of the writers mentioning the topic show only a passing interest in it. Many were indeed parish ministers, contributing to the great Statistical Accounts of the 1790s and 1840s or the parish and county histories that abounded in the latter part of the 19th century. Their interest centred essentially round local talking points. Characteristically, they viewed these in isolation, accepting traditions uncritically and making speculations regardless of inconsistency with neighbouring areas. Except when the writers were themselves connected with geology, there is very little evidence in the literature of attempts to assess local conclusions in terms of the general pattern of Scotland's former shorelines.

Although the archaeological side of the problem lay off the main line of 19th century development, this could certainly not be said of the purely geological aspect of work on Scottish sealevel change. Throughout the latter half of the century, the geomorphology and stratigraphy of the former shorelines of Scotland were subject to extensive enquiry, and much of this work reflected the best practice of 19th century field science.

The effectiveness of 19th century geological work on Scottish sea level changes owes much to the early participation of Scottish workers in the development of the general concepts on which modern geology is based. Hutton, Playfair and Lyell have already been mentioned. Following on Louis Agassiz' visit from Switzerland, and in particular the convincing arguments adduced by him at the Glasgow meeting of the British Association in 1840, the importance of glaciation by land ice quickly became widely accepted amongst Scottish geologists. This early and correct identification of the dominant process in the recent evolution of Scotland's landscape led to a period of remarkable activity in the latter half of the 19th century, that Sissons (in Craig 1965 p467) has gone so far as to describe as a "Golden Age" in the study of the Quaternary in Scotland. This would certainly seem to be true of the study of former shorelines, for work done in the first half of the present century shows little to match the achievement of the latter half of the 19th century, either in quantity or quality.

The most important mechanisms involved in the changes of land and sea levels were soon recognised. Within two years of Agassiz' visit, Maclaren (1842) suggested that the great quantities of water locked up in the ice sheets of the past most probably came from the oceans, thus lowering sea level substantially. It is now generally accepted that although this "glacio-eustatic" mechanism is by no means the only one affecting the overall level of the world's oceans (viz Chapter 4), it has certainly been dominant in the Quaternary.

In the same way, although "glacio-isostasy" is not the only mechanism that may produce differential warpings of the land and thus cause or complicate local changes of relative sea level, the reactions of the earth's crust to glacial loading and unloading has long been recognised as the other major factor in relative sea level changes in areas such as Scotland. Thomas Jamieson pioneered this view on the basis of careful observations in Scotland, and developed the idea in a long series of papers (1865, 1874, 1882, 1887, 1905, 1906, 1907; that of 1882 contains the most complete statment of his view). Beyond Scotland, his principle was applied first to Scandinavia (e.g. de Geer, 1888) and subsequently to most glaciated areas.

Although theoretical work of Janieson's calibre was important, in some respects it was outweighed in its influence on work in the first half of the 20th century by the late 19th century achievement in geological fieldwork. As Jamieson himself pointed out in 1906, some implications of the theoretical work tended to become lost in the mass of field observations. As indicated in Chapter 1, by this time the convenience of the "100 foot" and "25 foot raised beach" system of nomenclature had led to its use becoming so widespread that Jamieson described it (1906 p24) as an "article of faith". Although he considered then that field evidence was merely being assumed to accord with a convenient preconception, there is little in the geological or archaeological literature of the first half of the 20th century to show that his warning was heeded. As D.E. Smith has put it (1965), by the turn of the century the problem of Scotland's former shorelines seemed to have been solved.

There was thus indeed at that time a marked difference in the state of geological and archaeological opinion regarding sea level changes in Scotland.

Amongst geologists there was general acceptance of a clear and indeed elegantly simple system that appeared to account for the pattern of observations over the country as a whole. This "100-, 50- and 25 foot raised beach" concept had emerged in the course of confident and industrious exploration of Quaternary deposits, at the culmination of a period in which the work of Scottish geologists had achieved an international standing.

In contrast, the state of opinion on the archaeological date of coastal changes was in some disarray. Little attention had been paid to the overall pattern of the archaeological attributions in terms of the emerging geological model of the former shorelines, and, as indicated above, the literature is characterised by heterogeneous local speculations, often with obscure roots in oral traditions or based on hearsay, and frequently incompatible with opinions in neighbouring areas. Such opinions thus not only failed to present a coherent overall pattern but also individually tended to be more obviously vulnerable than the conclusions of the geological fieldworkers.

In view of the nature of this contrast, it seems hardly surprising that in the emergence of the 20th century pattern of thought on the archaeology of Scottish sea level changes, the dominant concepts should have come from the geological rather than the archaeological side.

Munro's paper of 1904 (quoted above) is characteristic of this trend. He attacked the hearsay evidence and uncontrolled speculation characteristic of many previous archaeological writers, and sought to base his own conclusions on well authenticated finds of artifacts. Nevertheless, he made no attempt to assess the geological evidence of sea level change, per se, but essentially based his conclusions on a complete acceptance of the current conventional classification of the shorelines. Indeed he entitled his paper, "On the date of the upheaval which caused the 25-feet Raised Beaches in Central Scotland".

Munro does not mention the "100 foot" and "50 foot Raised Beaches", for by then a "glacial" age was widely accepted for these, and they were thus not in general considered in relation to archaeological material (cf. Chapter 1). James Geikie had at one time (1881) split the carselands of Forth between "50 foot" and "25 foot " levels, and attributed some finds (e.g. canoes) to the former level, which he associated with a period of small glaciers in the Highlands (op.cit. p400-2). Although this view was cited sporadically in the literature until almost half a century later (until Callendar 1929), it was never generally accepted.

Thus, from the turn of the century, the literature on the archaeology of sea level change in Scotland has been dominated by the idea of a single raised shoreline within the period for which authenticated archaeological evidence is available, i.e. the postglacial or Holocene period.

Although in the present century various names have been proposed as alternatives to the traditional "25 foot Raised Beach " title, these have tended to refer to essentially the same concept, i.e. that of a single postglacial shoreline found most characteristically about the level of the 25 foot O.D. contour, but considered to be domed by isostasy so that its central portions lay above 40 foot O.D., while towards the periphery of Scotland it passed below the present sea level. The main names applied to this concept include:

"The Neolithic Raised Beach", Sollas (1904) and others;

"The Early Neolithic Raised Beach", Wright (1928);

"The Mesolithic Raised Beach", McCallien (1937);

"The Littorina Raised Beach", Movius (1942);

"The Early Post-Glacial Raised Beach", Lacaille (1954).

In the same way, although archaeological classifications varying from Palaeolithic to Roman were suggested in the first half of the 20th century, with very few exceptions (discussed at a later stage) they tended to refer to this same generalised concept of a single postglacial shoreline.

The Roman hypothesis does not appear to have survived Munro's attack of 1904. Hull's statement of 1903, noted above, seems to be its last appearance. Munro's paper also appears to have brought to an end most attempts to associate sea level changes with other parts of the iron-using period.

Munro himself (1898, 1899, 1904) described field evidence from

Maidens in Ayrshire which suggested that early Bronze Age artifacts had been involved in sea level change. The possibility of a Bronze Age date for the "25 foot Raised Beach" was later investigated in inconclusive excavations in Galloway reported in 1930 by Gregory, Ritchie, Kennedy and Leitch.

In general, however, the possibility of relative sea level change during any metal-using period has very seldom been seriously considered by 20th century workers,since Munro. Certainly, Callendar's short paper of 1929 seems to be the only serious attempt to martial Scottish archaeological evidence of sea level changes in terms of "prehistoric and recent times" as a whole. Otherwise, by World War I it appears to have been widely accepted that the "25 foot Raised Beach" represented the only change to be discussed, and that its context was the "Stone Age". Sollas had suggested that this was so in 1904, when Munro wrote, and this view has remained implicit in most archaeological discussions of sea level change in Scotland up to the present decade (viz Mercer 1968, publ. 1970).

Although the shift in interest from the very recent and Roman periods to the Stone Age appears to be a radical one, in several respects it followed on naturally from the pattern of 19th century development.

By the latter part of that century, interest in "Stone Age" ... Scotland was gathering momentum. Joseph Anderson had, for instance, published the part of his popular "Scotland in Pagan Times" dealing with the "Stone Age" in 1886, and steady growth of interest in the earlier periods is apparent in the Proceedings of the Society of Antiquaries of Scotland around the turn of the century. A few excavations had begun to stimulate interest in the relationship between non-metal-using communities and former sea levels as early as the 1870's, both on the east coast (e.g. D. Grieve 1873, Inchkeith) and in the west (e.g. Turner, 1871, Oban; Mapleton 1873, Crinan). In the final two decades of the 19th century this interest was considerably intensified by various reports, for example, from James Geikie (1881) and Sir William Turner (1889) on the Forth valley, from Gray on Campbeltown (1894), Symington Grieve (1882, 1885) and Anderson (1898) on Oronsay, and in particular Anderson (1895, 1898) and Turner (1895) on former sea caves at Oban.

These developments on the "Stone Age" front thus occurred not only during an era when attributions of sea level changes to the Roman or other very late periods were becoming discredited, but also coincided in time with the full emergence of the "25 foot raised beach" concept. This conjunction would appear to have had a lasting influence on the pattern of research in the 20th century.

A second factor appears also to have contributed towards the concentration on the "Stone Age" that is apparent in 20th century research on the archaeology of Scottish shorelines. Archaeologists concerned with pottery and metalware could in general achieve much more detailed and reliable typological distinctions than those dealing with chipped stone artifacts, and these distinctions permitted direct exploration of the relative chronology of such material. In the literature of the present century, it is notable that archaeologists in Scotland have tended to consider shoreline changes only when, faux de mieux, these might offer some possibility of establishing the relative dates of sites characterised only by stone or bone artifacts of problematic typology.

Hence, although many finds of other classes of objects have been made around the Scottish coastlands within what is now recognised as the range of the postglacial shorelines, their relationships to these features have hardly been discussed in this century. Indeed, from the Proceedings of the Society of Antiquaries of Scotland and the archive of local information collated by the

Ordnance Survey archaeological division (see Chapter 3), it is clear that ordinarily no specific investigation of this aspect of sites was made in the field by those who reported the very numerous Neolithic, Bronze Age and later burials and habitations, and other pottery and metalwork finds discovered on or near the coast.

Thus, once the work of the 1890s (see above) had prepared the ground by suggesting that "Stone Age" material was associated with the "25 foot raised beach", the lack of interest in shoreline-based chronologies <u>except</u> in the case of stone artifacts appears to have interacted with the widespread impression (cf. Chapter 1) that there was only a single postglacial shoreline to give rise to the assumption that it was unnecessary to consider sealevel changes in relation to sites represented by other classes of material.

Within the general heading "Stone Age", the artifacts that have been associated with the "25 foot raised beach" concept (either under that name or the alternatives listed above) have been repeatedly reassessed in terms of their possible cultural affiliations. Some of these attributions, relevant to present day problems, are discussed in Chapters 3 and 15. Many are now obsolete, and they will accordingly not be discussed in detail.

In general, however, it may be noted that even as late as 1930, Callendar considered the "25 foot raised beach" to be Upper Palaeolithic in date (e.g. Callendar 1928; letter to Gregory quoted in Gregory et al. 1930), at least at Campbeltown. Certainly Gray (1894) had described his finds there as Palaeolithic, and the Abbe Breuil in 1922 had also remarked on their Upper Palaeolithic <u>aspect</u>, though he apparently did not mean to imply as early as date as Callendar assumed (Lacaille 1954).

In general, however, for fully two-thirds of a century the "Raised Beach" was in the main associated with the earlier half of the

Postglacial, and with cultural traits that would in recent decades have been classified as "Mesolithic", although until the mid-thirties (viz McCallian 1937) this term was not prominent, and "Neolithic" or "Early Neolithic" tended to be used for the same characteristics.

The geologist W.B. Wright had used these latter terms in 1928, when he had proposed a qualification of the general scheme of dating. He suggested that on the system of archaeological classification current at that time it seemed that the "Neolithic or 25 foot beach" might be time-transgressive i.e. older at its centre, Oban ("Azilian"), than at its periphery (e.g. Campignian" at Larne). He postulated uplift occurring in the form of an expanding wave, and suggested that the distribution relative to the beach of "Neolithic implements of more advanced type ... including polished stone axe-heads" (p233) should be a subject of future research.

Although Wright republished the idea in geological publications on several occasions up to 1939, and it has since remained current among geologists and geomorphologists (e.g. Charlesworth 1957, Steers 1964, Synge & Stephens 1966, Stephens 1968) his concept appears never to have attracted attention among archaeologists.

In an earlier part of this chapter, the lack of contact between geologists and archaeologists interested in sea level change in the 19th century was noted. Wright's theory provides a graphic illustration of the seeming absence of any continuous dialogue between the disciplines in 20th century Scotland.

Indeed, on the archaeological side, it was not until forty years after Wright stated his idea that the concept of time transgressive shorelines seems to have appeared in a Scottish archaeological paper (Mercer 1969 publ. 1970). Mercer accepts the concept essentially on the authority of a survey of the literature made by Nichols, writing on palynology (1967). Nichols in turn cites Wright, but without detailing his argument, so it is not clear that Mercer is aware of the danger of circular arguments arising indirectly from obsolete data borrowed originally from his own discipline.

On the geological side, the geomorphologists Synge and Stephens show they are aware that Wright's archaeological argument must now be regarded as "questionable" (1966 p101), but as late as 1964 Steers stated without any qualification that (p5) "the association of Azilian and Campignian man with different parts of the 25-foot raised beach is a clear indication ..." of the validity of Wright's contention.

As the eminent Dutch marine geologist Keunen has put it (1954 p149), much circular reasoning appears to arise because specialist in other fields believe too much.

Similar examples of inadequate communication between the disciplines abound throughout the literature concerning the archaeology of Scotland's former shorelines, despite early expectations to the contrary. In the mid-19th century, Hugh Millar had written that "the antiquities piece on in natural sequence to the geology; and it seems but rational to indulge in the same sort of reasonnings regarding them;" while Sir Daniel Wilson had developed this by remarking of the geologist and archaeologist that "both deal with unwritten history ... and the detritus records archaeological as well as geological facts. The more recent strata are the legitimate property of both"..."the rude harpoon beside the bones of the whale, where now a child could ford the brawling stream... the boats beneath the City Cross of Glasgow: Here surely is common ground for the antiquary and geologist" (1851 p7,38). However, almost 90 years later, although McCallien (1937 p174) wrote "It is unnecessary to emphasise the interdependence of the two sciences, Geology and Prehistory," he had to add, "Nevertheless, in spite of the fact that this interdependence theoretically exists, it is remarkable how seldom there is any marked co-operation between the workers in these two fields. This is perhaps particularly so in Scotland."

In the absence of direct interdisciplinary contacts, borrowed ideas once adopted tended to remain unchallenged for long periods and to gain authority merely through usage. Thus, by the end of World War II, the association of the "25 foot raised beach" with postglacial hunters and gatherers who lacked the arts of agriculture had become so widespread that even V.G. Childe (1946; cf. Chapter 1, above) felt no need either to argue his statement or to name his authorities when stating that the date of such cultures was "guaranteed by geology".

The apogee of this view was perhaps reached in Lacaille's book of 1954, "The Stone Age in Scotland". This work embodied the most comprehensive expression of the view that the "25 foot raised beach" was "Early Post-Glacial" in date, and was associated with fully "Mesolithic" (in the sense of "pre-agricultural") material.

In the 1960s, not only was the traditional concept of a single "25 foot raised beach" questioned and rejected by many geologists and geomorphologists, but (as indicated in Chapter 1) some archaeologists, including Coles and Scott came to doubt whether much of the archaeological material associated with elevated marine deposits was as old as Lacaille and his predecessors had thought. In Scotland, as elsewhere in Europe and Asia (Piggott 1965) it would now seem injudicious to assume that the stone and bone technologies classed as "Mesolithic", and reflecting non-agricultural economies, necessarily pre-dated the local introduction of farming among other branches of the population. Thus not only have sites offering pottery or metalware been disregarded in terms of their potential relevance to sea level change, but consideration even of those sites represented only by stone or bone industries has been subject to preconceptions that now seem misleading.

Conclusion

In view of the unreliable nature of those geological and archaeological concepts that have dominated opinion on correlations between Scottish archaeological material and marine change for the greater part of this century, it seems advisable to re-examine the nature of the relationships between the available archaeological evidence and sealevel change at a fundamental level.

CHAPTER 3

The reliability of the available archaeological evidence

Examples of different types of Scottish archaeological evidence of potential relevance to shore line change, illustrating the points made in this chapter, are set out and discussed in Appendix I.

As shown in Chapter 1, it is now apparent that the concept of a "25 foot raised beach" is an oversimplification, and that the shorelines of Scotland have undergone a long and complex series of changes during the Holocene. The aim here is to assess how far the archaeological evidence at present available in Scotland is capable of resolving that complexity.

The closely spaced multiple shore features that have now been observed in different parts of the country suggest that alterations of the coast may have occurred as rapidly in Scotland as elsewhere on the western European seaboard. In a wide variety of localities there, many workers have found that during the Holocene episodes of relative sea level change have often occupied hundreds rather than thousands of years (see, for example, Bennema 1954, Berglund 1964, Lundqvist 1965, Marthinussen 1960, Mörner 1969, Müller 1962, inter alia).

It would thus seem that if archaeological evidence is to serve as a reliable basis for the analysis of relative sea level changes in Scotland, two requirements must be satisfied. Firstly, it must be possible to establish direct physical connections between antiquities and stratigraphic or geomorphological evidence of coastal change, and, secondly, relevant archaeolgical material must be of a kind that allows fine chronological distinctions to be drawn. The latter point will be considered first.

No radiocarbon dates are available for Scottish archaeological material directly involved in sea level change. The Hunterian Museum have recently obtained a C^{14} determination for midden material from Oronsay, but the sample was drawn from shells collected in the excavations of 1913 and the information available regarding their source on the site is insufficient to allow any but the most general conclusions regarding shoreline change: see (ii) below; also the Appendix to Chapter 14 for discussion and references.

Although C^{14} dates of archaeological sites more directly involved in coastal change have yet to be obtained, some dates relevant to artifacts similar in type to those that appear to be associated with marine changes are available. There seems reason to doubt, however, whether typological parallels are an adequate basis for distinguishing between shoreline changes which may be separated by only a few centuries. Sloping horizons recur in the temporal sequence in archaeology, and the relative timing of cultural changes can vary widely among different groups, even living in the same general area.

For example, V.G. Childe was interested in the survival of earlier technologies alongside later ones, and in his "Prehistory of Scotland" (1935, esp. p226 et seq.) he quotes many cases from Scotland, where, for example, stone tools of early aspect appear to have been in simultaneous use with bronze or iron artifacts, or bronze working traditions persisted long after the local introduction of iron.

The problem of adequate dating on typological grounds seems particularly acute in the case of the stone artifacts. The use of stone tools appears to have persisted virtually throughout the Holocene in Scotland (Sir Arthur Mitchell recorded (1880) that stone knives were still made and used in Shetland even up to Victorian times), but the age of a site is not necessarily apparent from the worked stone in itself. Thus, at sites with pottery, metalwork or other evidence of late date, the stone artifacts may not themselves be diagnostic of any particular period, or may indeed exhibit forms reminiscent of the Mesolithic. A selection of late coastal sites that illustrate this have been reported by Dalrymple (1866), Ewart & Curle (1908), Lacaille (1945,1951,1954), Laing & Huxley (1866), Lethbridge (1925,1927), Richardson (1902), Anc.Mon.Com. Scot. (1928), Tait (1868 & 1870), Wright & Peach (1911), among others.

Cases such as these suggest that it must be a matter of considerable difficulty to date sites securely when the typology of worked stone is the main or only line of argument available. This is confirmed by Coles major review article (1963) on Scottish material regarded as Mesolithic. As papers by Clark (1956) and Atkinson (1962) had also done previously, he indicates the uncertainty of many aspects of the classification of Scottish stone industries with regard to ultimate origins, cultural contacts, and hence typological chronology.

Coles indeed casts doubt on the validity even of commonly accepted basic descriptions of much coastal material. For example, after re-examination and statistical analysis of the finds, he suggests that although the Risga and Cnoc Sligeach, Oronsay, sites have in the past been regarded as Obanian (largely on the basis of their bone and antler components), in terms of their flint industries they "have sufficient divergencies to warrant a separate classification" (op.cit. p83).

He is also able to demonstrate that parallels that have often been claimed to exist between some Scottish stone industries and the Irish Larnian material are less close than has often been believed. Furthermore, he suggests convincingly that confusion that has arisen in Scotland becasue of the typological similarity of the Early Larnian and Ultimate Larnian industries has led to the attribution to some Scottish sites (e.g. Campbeltown, Ballantrae) of dates that are some two and a half millennia in error (cf Lacaille 1954, inter alia).

That reassessments of this radical nature have occurred within the last decade underline the real difficulty of evaluating many of the Scottish stone assemblages chronologically. Pottery, metal work and various other classes of artifact clearly offer greater potential for close typological dating.

It will be recalled from Chapter 2, however, that throughout the first half of the present century in Scotland archaeologists appear in general to have had recourse to concepts involving sea level change only where finds could not readily be attributed to dates on other grounds. This, together with the widespread assumption that there was only one "25 foot raised beach", appears to have contributed to a general neglect of the possibility of connections between shoreline change and pottery, metal work or other material of less problematic typology than the stone artifacts.

Thus, although the Proceedings of the Society of Antiquaries of Scotland and local publications show that very large numbers of archaeological finds of a great variety of types, have been made in the Scottish coast lands, with very few exceptions indeed (see (ii) below) the workers recording these finds have not reported on possible relationships to shoreline change, unless the material could be regarded as potentially "Stone Age" (or, latterly, "Mesolithic").

The multiple low-level Holocene shore features now recognised would appear to suggest that it is quite probable that changes of relative sea level continued in the periods when pottery and metal were in use. Potentially, therefore, there would appear to be some prospect of exploiting the relatively closely definable dates of some assemblages containing artifacts of these materials by making a search for previous neglected links between such finds and evidence of coastal change.

With some unfortunate exceptions (examples are noted in Appendix 1), the artifacts themselves generally survive in museums, or have at least been recorded in some detail. It is thus generally possible to re-examine their typological classification in terms of modern practice.

A fundamental problem arises in attempting any retrospective re-investigation of archaeological sites, however. The field-scientist dealing with the natural evidence of sea level change very often has at his disposal deposits of which hundreds, and indeed often millions, of tons survive. He can usually reinvestigate these at will when he comes to suspect that concepts have outrun the standard of previous observations. The archaeologist, on the other hand, must deal with, relatively speaking, exceedingly rare material, often set in unique assemblages and in unique stratigraphic contexts. He depends for much of his important material on finds made in haphazard fashion by non-archaeologists, and even when a thorough excavation is carried out by competent workers, the unique relationship of the artifacts to their context is irrevocably destroyed.

Thus, any attempt to reconstruct the context from which they came and establish whether or not physical connections had existed between the antiquities and the stratigraphic or geomorphological evidence of coastal change must rely heavily on reports in the literature of the circumstances of the find.

These reports are often inadequate for the purpose. As McCallien noted (Chapter 2 above), archaeologists and geologists have very seldom collaborated in Scotland. In the past, archaeologists, uninterested in shoreline change, almost invariably omitted from their accounts of finds any detailed description of those aspects of the natural stratigraphy and geomorphology of interest here. Even in the case of sites widely considered to be involved in sea level changes, it is difficult to evaluate the evidence since, as Sir Lindsay Scott once pointed out to the Prehistoric Society (Proceedings, 1948 p71), the bulk of the finds were made in an era when the details of stratigraphy of any kind received scant attention from archaeologists working in Scotland. If provision for chronological cross-checks with other lines of evidence (e.g. analysis of pollen of faunal remains) was not made at the time of original excavation, it can very seldom be secured satisfactorily in retrospect.

Clearly, it would be preferable to investigate fresh sites with the full modern techniques of environmental study, and the specific aim of elucidating their relationships to shore-level change. This approach is, however, hardly practicable in the short term. Not only would each individual excavation to these standards tend to be a lengthy and expensive project, but the apparent complexity of the shoreline changes suggests that each site investigated would be likely to supply information on only a fragment of the overall history of change during the Holocene. The actual finding of suitable sites also presents considerable problems. (See below, regarding a field search carried out by the writer.) The sites of potential relevance that have come to light in Scotland in recent years have proved on investigation to be of limited value.

For example, Mercer's site on Jura (1968 publ. 1970) has produced only stone artifacts which do not in themselves give a close indication of date (see Appendix 1). Candow's site at Morton in the Tentsmuir area of east Fife, which awaits full publication, has produced some material reminiscent of Star Carr and thus suggestive of a date fairly early in the Holocene. The writer, however, joined the excavation team and was able to establish that the site lay above the Holocene marine limit, and that there appeared to be no prospect of any detailed conclusions regarding marine change. The late butial site excavated by Colvin Greig in Largo was also visited, but its position between blown sand and rock head allowed only the most general conclusions to be drawn. The possible Roman site now being investigated by Graham and others at Granton (see Appendix) appears to be of even more limited potential.

It was thought that the shortcomings of the literature and the shortage of fresh sites might possibly be overcome by an intensive study of archaeological evidence in the Forth-Tay area, where (as indicated in Chapter 1, and as shown in detail in Chapter 13) recent work by J.B. Sissons, D.E. Smith, R.A. Cullingford and others has provided a great deal of data on shoreline morphology and Holocene stratigraphy. Accordingly, the archaeological literature of the area was searched and note was taken of all finds that might potentially lie within the Holocene marine limit between Arbroath and Dunbar. Over 300 such cases were considered.

In the investigation of these, the records of the archaeological division of the Ordnance Survey, both in Edinburgh and Chessington, proved invaluable. The personnel of this division are responsible for establishing whether the findspots of antiquities may be located sufficiently precisely to be published on Ordnance Survey maps. Their catalogue of reports from field officers contains much unpublished material, including the results of interviews with people involved in the original discoveries or excavations but now often long since dead.

All cases that seemed at all promising were followed up in the field by the present writer, and an extensive search was also made for any signs of new sites in the areas where the field maps of Sissons, Smith and Cullingford suggested that these might be informative in geological terms.

This approach proved unsuccessful. Some examples of material from the area will be given in Appendix 1, but the unprofitable parts of the investigation need not be detailed here. The reasons for the failure were in general difficulties of the type already discussed in this section.

Thus, often the older literature contained no useful details of the circumstances in which the finds were made, and local enquiries by the Ordnance Survey field officers and the writer failed to elicit any reliable information whatsoever. Not only was information on stratigraphy frequently absent or ambiguous, but in the majority of cases it proved impossible even to pinpoint the location where the find had been made, sufficiently securely to be confident as to whether it lay above or below shorelines mapped by the Forth-Tay team. Some antiquities were, for example, described only as being "found in the carse of Stirling", and even when a farm name was given the boundaries of that farm generally extended beyond a single shore level.

In most of the cases where any positive conclusion was possible, the archaeological material merely appeared to have lain on the surface, or in the overburden (peat or blown sand) of a particular shorelevel. Thus in general only very broadly defined "terminus ante quem" archaeological dates for the abandonment of shorelines by the sea could be proposed.

It thus became apparent that even a "total cull" of the archaeological material would not provide an adequate basis for resolving the complex chronology of the shoreline changes represented in the area. That this was so in the only substantial sector of Scotland yet covered so thoroughly by intensive geomorphological and stratigraphic studies suggested that the use of a similar approach elsewhere would tend to have even less prospect of success.

Nevertheless, in view of the inhospitable nature of the carselands of Forth and Tay (prior to the agricultural improvements of the last two and a half centuries) as compared to the favourable conditions for settlement on raised beaches in much of the rest of Scotland (e.g. Walton, 1966), it seemed possible that the Forth-Tay result was unduly discouraging.

Archaeological sites of possible relevance to marine change were therefore reviewed in geological terms for Scotland as a whole. Once again, however, the principle conclusion was the rarity of cases where the stratigraphic relationship of a site to evidence of marine change and also its archaeological date could both be simultaneously established with adequate reliability for the purpose in hand. It became clear that, provided it was feasible, it would be preferable to seek some non-archaeological method of resolving the marine changes.

The detailed grounds for this conclusion are illustrated in the Appendix.

A non-archaeolgical approach would also seem desirable for an additional reason. As indicated in Chapter 2, since the beginning of the present century thinking on the archaeology of sea level change in Scotland has been dominated by the concept that there was only one raised beach and that this was of "Stone Age" (latterly specifically "Mesolithic") date.

Both the general pattern and the detailed phrasing in much of the literature cited in Chapter 2 and Appendix 1 suggests strongly that throughout the first half of the 20th century this preconception has not only influenced the discussion of Scottish archaeological material, but has also tended to condition the nature of field investigations, affecting the questions asked during excavation and the way that observations were interpreted and reported. This tendency seems detectable as late as 1954, in Lacaille's major work "The Stone Age in Scotland". The criticisms made in the Appendix regarding his handling of the evidence from Camphill, Stannergate, Freswick, Duntroon and Ballantrae provide examples.

Because of the uncertainty attending so many aspects of the archaeological evidence, even in cases where there is good reason to suspect that circular arguments have influenced specific reports it is difficult to be confident that in attempting to allow for this one is not merely superimposing one's own preconceptions.

On balance, then, in order to contribute towards a useful framework for future enquiries into the archaeology of shore line change in Scotland, it would seem advisable to segregate the archaeological and other evidence, and to concentrate initially on the natural history of the marine changes involved.

CHAPTER 4

The approach adopted in the investigation of Holocene marine change

Introduction

In Chapters 1 to 3, the unsatisfactory state of current knowledge regarding the relationship between Holocene marine change and Scottish arehaeological material has been indicated. The present chapter is in three parts. After reviewing what should now be attempted (part i), problems and approaches are discussed in (ii) and the course adopted is defined in (iii).

i) Aims

The basic objective in the present study is to aid those interested in the prehistoric geography of Scotland's coastlands by developing some more profitable basis for future investigations than that presented by the obsolescent concept of the "25 foot raised beach".

The complex nature of the shoreline changes now becoming apparent in Scotland, as elsewhere in Europe, has been noted. The experience of the team working in the Forth-Tay area has shown that widely spaced height measurements are inadequate to resolve these complexities in a reliable way (Chapter 1). The technique that this team uses, that of tracing shoreline remnants continuously by very closely spaced precise measurements, is clearly valuable but both its expense and the shortcomings of much morphological evidence elsewhere in the country makes it unlikely that this approach alone will provide what archaeologists require: i.e. a reliable series of generalisations applicable to sites that may come to light anywhere around the Scottish coasts.

The very feasibility of such generalisations is indeed now in doubt. As indicated in Chapter 1, at the present state of knowledge, considerable uncertainty exists as to how far events recognised in one area are likely to correspond to those registered elsewhere. It was suggested there that this question is central to the future development of concepts relating marine change and Scottish prehistory. Thus, if local factors tend to predominate, the value of shoreline changes for developing chronological frameworks for archaeological material would be minimal, whereas the opposite would be the case if eustatic ocean level variations tended to synchronise the changes in different areas.

It is accordingly proposed that instead of extending the area covered intensively by morphological measurements (by a necessarily limited amount), it would seem a more useful contribution to current knowledge to seek an alternative, but non-archaeological approach to the problem of elucidating how far the complexities of relative sea level change in areas such as Scotland are likely to reflect local factors, and how far synchronous eustatic influence.

An approach of this nature would have the value of being of potential interest not only in the context of specific Scottish problems, but also in terms of the general understanding of the nature of marine change and coastal landscape evolution during the Holocene.

ii) Discussion

The field evidence surviving in any area tends to record the changes of relative level resulting from the interaction of movements of the ocean's surface with changes in the configuration of the land, but without giving any immediate indication of their specific roles in the interplay there. It is thus a matter of some difficulty to distinguish between the eustatic and local components. The considerable amount of controversy still surrounding the analysis of this interplay is illustrated by Figure 4.1. This shows graphs that have been proposed in recent years in attempts to isolate the eustatic element (the individual curves are identified and discussed in Chapter 12).

The problem of analysis is complicated by the number of factors that appear to be involved both in eustatic ocean variations and in local coastal influences. Thus, although as indicated in Chapter 2 glacio-eustasy and glacio-isostasy were correctly identified at an early date as the dominant processes in areas such as Scotland, those are by no means the only factors. Eustasy will be considered first.

Other mechanisms besides glacio-eustasy, that are likely to have produced eustatic changes have been reviewed comprehensively by various writers (e.g. Charlesworth 1957, Flint 1957, Fairbridge 1961) and primary references to the extensive literature on each of these processes thus need not be cited here.

Most eustatic mechanisms may be classified under two headings, according to whether they produce:

a) Changes in the volume of the ocean and sea basins through several types of tectonic movements and isostatic adjustments, and various other factors which include for example sediment infill.

b) Changes in the volume of marine water through not only glacier variations but such factors as variations in the amount of water held inland in lakes or the addition of juvenile water from volcanoes; volume changes also result from changes in the nature of the water column itself (e.g. variations in temperature and salinity).

Some of these mechanisms are clearly of minor importance with regard to the Holocene (e.g. the slower epeirogenetic movements of ocean basins, sediment infill or the addition of juvenile water). In general, however, there is a shortage of reliable quantitative data on their relative importance. As Flint pointed out, (op.cit.), the decanting into the main ocean not only of lakes created temporarily on deglaciation, but also of water from shallow seas in regions of crustal uplift, is ordinarily neglected. Yet this involves water-bodies as extensive as the former Lake Agassiz, the Baltic and Hudson Bay. Furthermore, recent papers by Bloom (1967), Higgins (1968) and Matthews (1969), suggest that hydroisostatic stretching of the ocean basins, as well as the local response of coasts to water-loading, (see below) was not negligible during the Holocene. Similarly, calculations by Schofield (1967) have drawn attention to the desirability of taking into account the changes of salinity attending the release of glacial meltwater into the seas, while others by Munk (pers. comm. to Fairbridge 1961) suggest that the same is true of changes in water temperature.

Thus, although it is well established that glacio-eustasy has been the dominant process in ocean level variation in the Quaternary, and several recent eustatic calculations based on changes in glacier ice volume (see Chapter 12) appear to offer an acceptable general picture of the overall eustatic trend in the Holocene, it would seem that insufficient quantitative data is available to permit the interactions of the whole complex of eustatic mechanisms involved to be calculated in detail.

Since the complexity of the Scottish former shorelines (see Chapter 1) suggests that the pattern of eustatic change would have to be known in considerable detail before the nature of the interplay between oceanic and local factors could be adequately evaluated, some other approach offering a higher degree of revolution is required.

Many workers have sought to identify areas of crustal stability, which would serve as direct "measuring marks" against which eustatic changes might be traced. In recent years, they have included (amongst others): Auer (1958), Fairbridge (1961), Shepard (1963), Scholl (1964), Milliman & Emery (1968). Opinion, however, appears to be turning against this appreach. Newman and Munsart, for example, have gone so far as to state, "... we suspect that coastal stability is a myth. We doubt that any coast has remained stable during the Pleistocene Epoch". (1968, p95)

Certainly, recent geophysical work on hydro-isostasy would appear to endorse such doubts by indicating that the continental margins respond to loads of the order involved in Holocene sea level change (e.g. Bloom 1965 and 1967, Broeker and Kaufman 1965, Crittenden 1963, Higgins 1968, Matthews 1969). As Mörner has recently remarked (1969 p420) "hydro-isostasy is a fact which must now be taken into account. This means that the transgressed shelf areas most probably have sunk hydro-isostatically. Thus it must be very hard to find a real 'stable area'".

At the present state of knowledge, it would therefore seem advisable to seek an approach that does not rely on the concept of "stable areas", but instead is based on the probability that all available data may involve movements of both land and sea surfaces.

The mechanisms that may give rise to changes in the configuration of the land in addition to glacio-isostacy, are again both numerous and difficult to evaluate in quantitative terms. As with the eustatic mechanisms, they have been reviewed comprehensively, for instance, by Charlesworth (1957), Jelgersma (1961), Higgins (1965), Stephens and Synge (in Dury 1966) and, notably, in the 1954 Symposium on Quaternary Changes in Level (published in Geol. en Mijnb. N.S.16 No.6), and the 1966 Symposium on Recent Critical Movements (published in Ann.Acad.Sci.Fenn.A III 90). As in the case of eustatic mechanisms, the treatment here may accordingly be brief.

Again, the mechanisms may be divided into two principal classes, in this case according to whether they involve:

a) <u>Movements of the earth's crust itself</u>

b) <u>Changes in the superficial deposits</u>

In addition to glacio-isostacy, the former of these classes includes phenomena as diverse as hydro-isostasy (noted above) and the continuation into the Holocene of epeirogenetic movements of considerable geological antiquity

(Lyustikh 1957, and Hallam 1963, amongst others, have suggested that the importance of the latter factor has been seriously underestimated). The difficulty of evaluating the interaction of different crust-displacing processes quantitatively with regard to time was emphasised at both Symposia, and the divergences of opinion on recent movements of Scotland (e.g. Valentin 1953, Hafemann 1954, Taylor & Smalley 1969) illustrates the uncertainty of extrapolating from the limited amount of observational data available when the relative importance of the mechanisms involved is unclear.

Changes in the superficial deposits may be of various kinds. In particular, compaction of thick unconsolidated deposits or peat beds may cause substantial subsidence. Although a considerable body of data on subsidence characteristics of various materials has been amassed by geologists and civil engineers, because of the changing rate at which it occurs and the number of variables involved it remains difficult to reconstruct the history of past changes due to this cause (Fisk 1956, Jelgersma 1961 & 1966, van Veen 1954 & 1957, Weller 1959, Wiggers 1954).

The problem of isolating eustatic from non-eustatic elements in coastal change is further complicated by other factors. For example, the creation or erosion of barrier bars (Fisk 1956, Leontyev & Nikiforov 1965), or similar changes, may give rise to stratigraphy erroneously suggestive of a change in level.

In view of the considerable uncertainties that may thus exist at any one locality, it would seem desirable to approach the problem of assessing the role of the eustatic component in relative change by comparing the patterns of change recorded at a large number of sites, preferably characterised by diverse land-movement regimes and varied coastal environments. If the histories of change prove dissimilar, the influence of eustatic variations on the details of coastal change may be assumed to be minimal. If, however, the tendency is towards marked similarity, then there would appear to be a strong case for believing that the ubiquitous influence of eustatic changes in ocean level is responsible.

Problems of approaches based on morphological measurements have already been mentioned in Chapter 1 and in (i) above. Certainly a comparative study on an extensive basis, as proposed here, would seem difficult to accomplish adequately on morphological grounds alone, because of the difficulty of ascertaining whether features in different localities were in fact equivalent.

For example, although attempts have been made to codify the characteristic height relationships of different types of shore features to mean sea level (e.g. Hideo Mii 1962, Fairbridge 1961 p140), these suggestions seem of limited practical value in the comparison of ancient erosional and depositional features in different areas, because of the complicating influence of variations in exposure (e.g.Hornsten 1964), tidal range, terrace composition and gradient, and such factors as the availability of constructional materials. Correction factors are not necessarily provided by study of the height relationships of present day shore features at each site to contemporary mean sea level, because often each of the factors listed above varied from time to time at the same site, as the relationship between the sea level and the local and regional topography changed. Regarding tidal range, for instance, Thom (1960) has pointed out that whereas the natural frequency of the oscillation of the water in the North Sea basin is now about 19 hours, when the basin was smaller (as in the earlier Holocene) the period would be nearer 12 hours, giving rise to a substantially greater tidal effect then. Van Straaten (1954) has demonstrated that quite different relative sea level graphs are obtained if different tidal ranges are assumed while interpreting heighted data from even a single site.

The possibilities of confusion arising while attempting to allow for such factors in the course of an extensive comparative study would seem to be great, even if adequate measurements of the present heights of the ancient shoreline features were available.

It would thus seem desirable that the criterion of comparison used in evaluating the eustatic/non-eustatic interaction should be one that would allow the equivalence of features in diverse localities to be investigated without requiring them to be equated initially on grounds of height and morphology.

In theory at least, many of the problems indicated here might be circumvented by an approach based primarily on direct investigation of the timing of changes in marine influence, rather than on examination of the three-dimensional patterns of their results.

Thus, comparison of the dates of events in a wide range of localities could yield an indication of the extent to which eustatic influence was effective, relative to more localised factors. Since eustatic ocean level variations characteristically occur simultaneously everywhere, if a high degree of synchronism in coastal changes was present overall, despite a diverse range of land movement regimes and coastal environments, this would suggest that eustatic control was dominant. On the other hand, if this was not so and local factors predominated in determining the details of changes in marine influence, any geographical patterns in the timing of changes might help to identify which of the non-eustatic factors tended to be most important in different types of area, and the validity of theories such as that of Wright (noted in Chapter 2) could be evaluated.

In the initial stage of a survey of this nature, based on timing, it would be sufficient merely to know that a considerable variety of coastal and geophysical conditions was represented in the data, and the problematic assumptions and practical difficulties of height measurement and interpretation involved in correlating features morphologically could be avoided until the cardinal question of the relative roles of the local and eustatic components had been determined.

Once the survey of timing was complete, however, its results concerning such matters as whether the Holocene eustatic curve was smooth or oscillated (and, if the latter, how many generally distinguishable events took place) should in turn facilitate analysis of the height patterns of the data.

For such an approach to be viable, however, an adequate method of dating coastal changes is required. Since a fundamental reassessment is needed, this method should clearly be independent of past morphological concepts, such as that of the "25 foot raised beach". The inadequacies of archaeological material for timing complex successions of marine changes have already been discussed. Techniques based on the analysis of flora and fauna, for example pollen and diatom analysis, share with archaeological dating the disadvantages of dealing with complexes of material often characterised by broad time bands in any one area, and by geographical distributions with boundaries that vary in date from place to place (cf Oakley 1966, Zeuner 1958).

Radiocarbon dating is not subject to these problems and at present appears to offer the most effective system of relative dating available for Holocene material that can not be dated historically. With very few exceptions (e.g. Milǒjćic 1967), most archaeologists and Quaternary scientists accept the general validity of the technique, which has been attested by over twenty years of use since first introduced by Libby in 1949 (Libby 1951; Neustupny 1970, on the results of the XII Nobel Symposium, held at Uppsala 1969 on progress in radiocarbon dating).

The basis of the method has frequently been described, and accordingly need only be outlined here (Libby, 1965, gives a concise exposition of the principles). The method is based on measuring the C^{14} activity of biogenic material. Radiocarbon is produced from nitrogen in the upper atmosphere by a neutron flux resulting from cosmic ray bombardment. The C^{14} is oxidised to radioactive carbon dioxide, which mixes with the ordinary atmospheric carbon dioxide. It is incorporated in the biosphere, and reaches equilibrium with the atmospheric, freshwater and oceanic carbon reservoirs. Organisms maintain equilibrium with the radiocarbon in their environment while alive, but on death the radiocarbon in their remains tends not to be replaced, and decays at a characteristic rate (defined for convenience in terms of the time in which half the radioactivity in any given mass will be lost: the "half-life", see Chapter 5). The relative age of a sample is calculated from a short term count of radioactive emissions from the fossil sample, taking into account the half life of C^{14}, the contemporary count, and background radiation. As shown in Chapter 5, following substantial international co-operation in laboratory cross-calibration, it is now possible to reduce determinations made by different laboratories to a common standard, so that all are directly comparable.

Several assumptions are implicit in the use of radiocarbon assay for dating. One of the most fundamental of these in the present context is that the atmospheric source of radiocarbon is geographically constant in its specific activity, so that determinations from different sites are in fact comparable. After discussing investigations by Libby, de Vries, Tauber, Jansen, Lerman, Mook and Vogel, all reported at Uppsala, Neustupny concluded that although there is a systematic difference between the overall concentrations in the Southern and Northern Hemispheres (more C^{14} being absorbed by the greater ocean surface in the south) this is constant and amounts to only about 40 years, while (1970 p44) "it has been ascertained beyond any doubt that the variations in radiocarbon concentration are a worldwide phenomenon", synchronous and of the same amplitude all over the earth.

Although the substantiation of Libby's original assumption that this was so confirms the validity of C^{14} assay as a method of relative dating, it has become clear that the equation of radiocarbon "years" with absolute siderial years is not so simple as was at first thought. The half-life has been measured several times and its value is well established at about 5730±30 years (Johnson 1965; see Chapter 5 concerning the continued use of the original Libby standard of 5568 years). However, it has become evident that the past concentration of radiocarbon in the atmosphere has not remained constant, resulting in non-linearity of radiocarbon dates if plotted against calendar years.

The calibration of the radiocarbon timescale for the Holocene in terms of siderial years is now well advanced (viz Renfrew 1970) on the basis of cross reference to a variety of other techniques, particularly dendrochronology (see Chapter 5). This work is, however, as yet incomplete.

Radiocarbon assay thus remains for the moment essentially a method for determining the relative date of samples. Its reliability and accuracy in doing this will be assessed in detail in terms of the aims of the present study in the chapter which follows. At this point, however, it must be noted that the validity of any individual C^{14} date depends on a further major assumption. This is that the material assayed has not been contaminated by the addition of older or younger material, either by natural processes or artificially in the course of field sampling, storage or laboratory handling. This is often difficult or impossible to ascertain in individual cases. Therefore, if the investigation of the relative importance of eustatic and other factors in Holocene coastal change is to yield reliable results on the basis of comparisons in timing, it would seem advisable to minimise the influence of contaminated samples by basing the results on a substantial number of radiocarbon determinations.

So far, relatively few radiocarbon dates directly relevant to Holocene changes of marine influence are available in Scotland itself. These are listed in full in the Appendix to Chapter 14. Although in all 40 dates of potential relevance are available, as shown in Chapters 13 and 14 a proportion of these are in fact uninformative. The complexity of the Holocene changes revealed, for instance, by the Forth-Tay study, suggests that the uneven distribution of of the remaining dates, (both through time and in terms of geographical position and the range of conditions represented) would not of itself give adequate coverage to allow the roles of eustatic and non-eustatic components to be resolved. In order to secure a result possessing a degree of reliability commensurate to the importance of the problem, a considerably larger body of data seems desirable.

When Fairbridge set out in 1961 to investigate radiocarbon dates referring to sealevel change, he commented (1961 p137) on the dearth, at that time, of relevant determinations and attempted to collate dates on a worldwide scale. The possibility of seeking to define the eustatic element by following his approach and working in terms of C^{14} dates drawn from the whole world was considered. It was however concluded that this should if possible be avoided, on various grounds. These were in part theoretical (viz Schofield 1967; Munk & Revelle 1952; Young 1953; Jones 1961). In practical terms, it was considered that it would be very difficult to interpret the results of a chronological survey of widely dispersed world data in an adequate way, because of the shortage of basic information on the Holocene development of many of the world's coastlines.

A pilot study showed that the number of radiocarbon dates relevant to marine change has increased substantially in the decade since Fairbridge wrote. Furthermore, western Europe appears to contain the largest concentration now available, yielding almost a thousand C^{14} determinations directly relevant to Holocene marine changes.

It was decided that it would be preferable for present purposes to concentrate on this area. The number of determinations seemed sufficiently substantial to reduce the significance of random errors of dating (because of contamination of samples, say). Furthermore, the long established interest in research into coastal changes, both in Scandinavia and in the North Sea countries, meant that an extensive literature was available to aid the interpretation of the C^{14} data. More important than factors such as these, however, in influencing the decision to concentrate on the western seaboard of Europe was the way in which the range of land movement regimes and of coastal environments represented there fully embraced the spectrum of conditions found in Scotland during the Holocene. Indeed, the seaboard from Arctic Norway to Biscay by way of the Baltic, North Sea, Channel and Irish Sea coasts, includes a range of combinations of conditions considerably broader than that found in Scotland (see Chapter 11). This offered valuable control for the assessment of how far coastal changes are likely to be synchronous in different localities within the more restricted part of the range represented around the Scottish coasts.

With so many C^{14} dates available, the danger exists that any approach giving scope for eclectic handling of the data might result merely in the selection of determinations that accorded with preconceptions of the nature of postglacial marine change. Clearly, an holistic approach encompassing all the available data is desirable, and an appropriate statistical technique is required to secure the objective handling of the large body of information involved.

iii) The course adopted

In summary, the approach adopted may thus be defined as involving the following elements:

a) Segregation of archaeological and non-archaeological evidence, and initial concentration on the latter.

b) Avoidance of preconceptions regarding the correlation of former shorelines in different localities, because such correlations often seem based on inadequate morphological measurements.

c) Exploitation of the ubiquitous and synchronous nature of eustatic episodes, by using a direct investigation of the timing of coastal changes to establish how far these tend to reflect sommon eustatic or independent local factors.

d) Implementation of this investigation through the medium of a large number of C^{14} determinations, reduced to a common standard in light of laboratory cross-calibration, and employed as a system of relative dating.

e) Avoidance of an eclectic approach to the C^{14} data; use of descriptive statistics in an attempt to secure an objective initial hypothesis.

f) Detailed examination of this hypothesis in terms of the stratigraphy, geographical distribution and height/depth pattern of the data.

g) Execution of this investigation of the nature of Holocene marine change in terms of the seaboard of western Europe north of Biscay, initially omitting Scotland.

h) Proposal of an eustatic curve consistent with the results of this survey.

These elements form the substance of Chapters 5 to 12 of the thesis. The way in which these are organised is indicated in the introduction to Chapter 5.

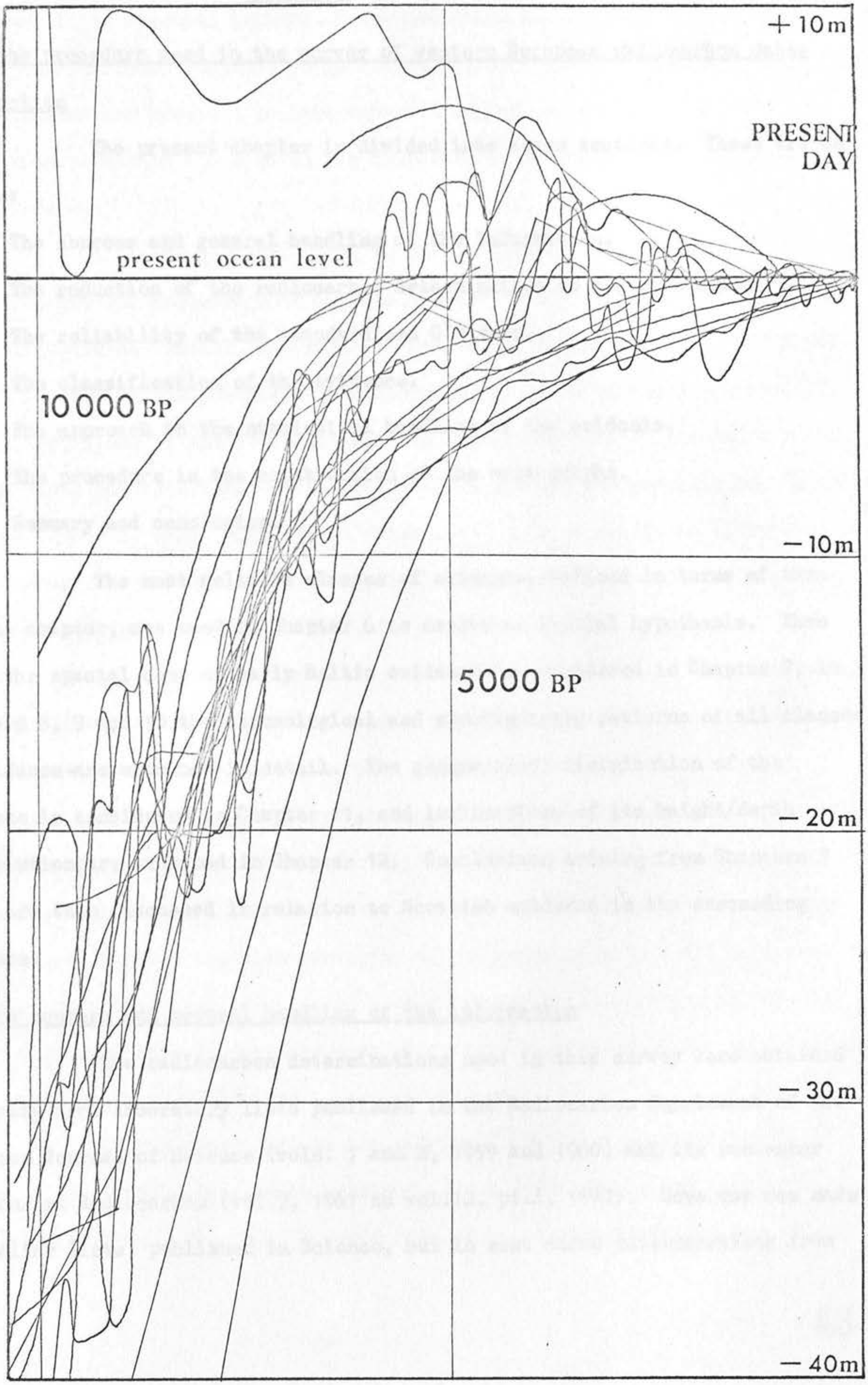
Chapters 13, 14 and 15 are concerned respectively with:

i) Investigation of the compatibility of the detailed data available for the Forth-Tay area of Scotland with (c-g) and (h) above.

j) Discussion of Holocene relative sea level changes in Scotland as a whole.

k) Assessment of implications regarding concepts involving Scottish archaeological material.

Figure 4.1 Recently published graphs of Holocene marine change, superimposed at a uniform scale. (These are identified in 12.2 to 12.12)


+ 10m
PRESENT DAY
present ocean level
10 000 BP
5000 BP
— 10m
— 20m
— 30m
— 40m

CHAPTER 5

The procedure used in the survey of western European radiocarbon dates

Introduction

The present chapter is divided into seven sections. These are as follows:

i) The sources and general handling of the information.

ii) The reduction of the radiocarbon determination to a common standard.

iii) The reliability of the standardised C^{14} data.

iv) The classification of the evidence.

v) The approach to the statistical handling of the evidence.

vi) The procedure in the construction of the main graphs.

vii) Summary and conclusion.

The most reliable classes of evidence, defined in terms of the present chapter, are used in Chapter 6 to derive an initial hypothesis. Then after the special case of early Baltic evidence is considered in Chapter 7, in Chapters 8, 9 and 10 the chronological and stratigraphic patterns of all classes of evidence are examined in detail. The geographical distribution of the evidence is considered in Chapter 11, and implications of its height/depth distribution are examined in Chapter 12. Conclusions arising from Chapters 5 to 12 are then discussed in relation to Scottish evidence in the succeeding chapters.

i) The sources and general handling of the information

The radiocarbon determinations used in this survey were obtained primarily from laboratory lists published in the Radiocarbon Supplement of the American Journal of Science (vols. 1 and 2, 1959 and 1960) and its successor the Journal Radiocarbon (vo..3, 1961 to vol.12, pt.I, 1970). Some use was made of earlier lists, published in Science, but in most cases determinations from these were later rejected because of uncertainties in relating them to the current international standard (ref. Deevey, Flint & Rouse, 1967. See below.) Dates obtained too recently to have appeared in the current issue of Radiocarbon have been included as far as possible, provided they conformed to that standard.

A classified card index to the laboratory lists has been published, but the criteria used in this did not ensure that all relevant determinations would be traced. A systematic search was therefore made through all entries in each of the published laboratory lists. This proved a substantial undertaking, since it involved unclassified lists published over a dozen years by some sixty laboratories. It is however difficult to see how this search could have been avoided, since many of the dates have not been published elsewhere. It was necessary to consider all laboratories, because European material has frequently been dated by laboratories as far afield as Japan.

As relevant entries were located, they were copied Xerographically. With the numbers involved this was expensive, but it was considered necessary in the interests of accuracy on two levels. Firstly, the amount of numerical information needed from each entry was large (often as many as 60 digits, referring to the date itself, standard deviation, laboratory number, latitude and longitude, altitude, bed thickness, etc.) and it seemed unrealistic to hope to avoid errors when dealing with something of the order of a thousand such entries. Secondly, when dealing with as large a mass of complex information as this it seemed very desirable to preserve each entry intact, instead of taking notes which might unwittingly be influenced by preconceptions.

It was clear that in the course of analysis, many different characteristics would have to be examined, first individually and then in different combinations. The entries would have to be re-organised in terms of not only their date but their geographical location, material, stratigraphic context, bed thickness, altitude, relationship to other dates at the same site, and so on. It seemed advisable to find a more efficient system of access to the data than the writing out of multiple key lists for each of the combinations required. It was concluded that some form of punched card sorting would be the answer. Edge punched 8" x 5" Copeland Chatterton P2 CC14 cards were used, since this size allowed the Xerox copies of the original entries to be affixed directly to the cards. A "Superimposed" coding system was used to minimise the amount of punching necessary. The cards were cross-indexed to a reference file of Xerox copies of other publications amplifying the laboratory entries for the dates. The principal works consulted are listed in the bibliography.

ii) The reduction of the radiocarbon determinations to a common standard

It was emphasised in the last chapter that the relationship between radiocarbon "dates" and calendar years has yet to be adequately established, and that accordingly radiocarbon dating must be regarded as a system of relative, rather than absolute, dating. All radiocarbon determinations mentioned in the present work should therefore be regarded in this light. Although for convenience the convention of expressing the determination in "years" is maintained, no specific attribution to calendar time is intended. The 1969 Nobel Conference made it clear that the elucidation of the relationship between the radiocarbon scale and calendar years may be expected to progress greatly in both scope and precision in the next few years. In light of this, the writer considers that it would be premature to modify the figures used here in terms of what is still an incomplete picture of that relationship. At this stage it seems more desirable to seek to produce a study based on internally consistent relative dating, since it would almost certainly be easier to uprate this retrospectively to the standards of later knowledge than it would be to disentangle premature attempts to equate the differing time scales. The aim here has therefore been to reduce all the determinations used to common standards, and to ensure that the standards adopted conform to well established international usage.

In 1962, Godwin stated in a letter to Nature (vol.195, No. 4845, p.984) that 5730 ± 40, the mean of three new determinations of the half-life of radiocarbon, should be regarded as the best value obtainable. This is still accepted (Radiocarbon, 1970). However, the Cambridge and Washington Conferences of 1962 to 1965 recommended that all dates should continue to be published on the basis of Libby's original half-life of 5570± 30 years, to avoid confusion from the use of two standards. The 1969 Nobel Conference reaffirmed this. Since the variations in the radiocarbon scale are clearly not linear relative to calendar time (e.g. Stuiver & Suess, 1966, Suess 1967), the "better" mean half-life is of limited value. Some writers have however employed it in dealing with dates included in the present survey (notably Berglund, 1964). All such cases have however been reduced to the Libby standard. Below, whenever the other half-life value is taken into account, this is specifically stipulated. Otherwise, the Libby value is used as standard, throughout the present study. All dates are stated in radiocarbon years before present (B.P.), where "present" is taken as A.D.1950.

In the early 1960s, it became clear from independent measurements of duplicate samples that discrepancies existed in the calibration of radiocarbon laboratories. Sometimes different equipment in the same laboratory gave different sets of results and an individual laboratory published a retrospective checklist with corrections for its previously published dates. (e.g. Groningen IV, Radiocarbon Vol.5, 1963.) In many cases it also became clear that the reference samples used in different laboratories did not conform to a common standard. An international programme of cross-calibration was carried out in terms of an agreed oxalic acid standard supplied by the U.S. National Bureau of Standards, and the opportunity was taken to make a general revision of all dates published internationally between 1950 and 1965. Dates published in Radiocarbon from 1966 onwards have conformed to the standards established in that revision. The retrospective survey was completed in 1967 and was published by the American Journal of Science as a checklist. This appeared in 1968, edited by Deevey, Flint & Rouse under the title "Radiocarbon Measurements: Comprehensive Index, 1950-1965". All dates published in this period were confirmed, modified, or declared invalid. Following correspondence with the board of Radiocarbon at Yale (Berney, pers.comm.) the writer concluded that the changes should be fully incorporated in his body of data. Accordingly dates considered unreliable in the Index were discarded and all other punched cards were brought up to standard. It would seem that a larger body of internally consistent radiocarbon dates has been assembled here than has been used in any previous study of European sealevel change.

The Journal Radiocarbon makes no standard correction on determinations made from seashells. The introduction to the Index notes the differences between these and determinations from terrestrial organic carbon, and goes on to state that the practice is to report them as normal dates but "with mental reservations that can be made explicit only if the oceanographic situation is known" (p.2). Any correction of shell dates thus tends to be on a less firm basis than corrections of the main types given in the Index. Investigation of the magnitude of differences likely to prevail along the western European seaboard suggested however that some attempt should be made to reduce the shell dates used in the present study to a common standard with the dates of terrestrially derived material.

Marine shells are differently affected by isotopic fractionation, and by other errors affected by the ultimate source of the isotope. Fractionation alone makes a small difference, at most 5% (Deevey et al, 1967). The main difference arises from the fact that the sea-surface carbonate used by molluscs in forming shell is generally depleted in C^{14} below the assumed equilibrium value: i.e. seawater generally has an apparent age in excess of its actual age, and shell dates thus tend to appear older than dates of terrestrially derived organic carbon to which they should correspond.

The apparent age of seawater varies somewhat from place to place, according to oceanographic criteria, such as the amount of mixing that takes place between deeper and shallower layers in different areas. Data relevant to the Western seaboard of Europe are available. The greatest differences represented in the period of interest are of the order of 500 years. This is so for recent material (e.g. U-607, live Patella vulgata, collected 1935 (530 ± 72) and also for material immediately preceding the Holocene. For instance, wood and shells from the same Late Glacial deposit at Blomvåg in Norway gave

shells	12700 ± 350
wood	12200 ± 350
	500

500 years is in accordance with the determinations for Arctic water from the East Greenland current (Fonselius & Östlund, 1959). However, most differences are considerably less than this.

Fallout from nuclear weapons testing has artificially minimised the difference in some cases. For instance, seaweed from the Skagerrak, collected in 1959, showed a level of radioactivity <u>above</u> the equilibrium level (St 428) and although samples from the same area collected in 1905 and 1950 showed differences of 280 ± 56 (St 369) and 385 ± 56 (St 363), respectively, some collected in 1958 showed only 55 ± 48 (St 364). If figures which appear to imply fallout contamination are omitted, most determinations for surface water fall within a range centred around 300 years apparent age. E.g. Norwegian Sea: St 458, 278 ± 88; St 460, 320 ± 72; Skagerrak: St 639, 280 ± 56. Although one determination from the Bay of Biscay gave a value of 179 ± 56 (St 421), Broeker (1957) concluded that the average for North Atlantic surface water was 320 years. A correction of about - 300 years would therefore seem to be indicated.

Varying corrections have been suggested in the past for shell dates relevant to the present survey. The differences between these proposed corrections were based not on detailed information but on slightly varying interpretations of general information of the type just listed. Until more detail becomes available, in order to avoid creating spurious differences of this kind it seems advisable to adopt one general correction and apply it uniformly.

Morner (1969) has already applied a uniform correction to a substantial number of shell determinations relevant to the present survey. His value of - 305 ± 25 appears to accord well with available information, and it has accordingly been used throughout, <u>except</u> for Baltic shell dates.

In comparison with the Atlantic, the Baltic is dominated by input of terrestrial material and the sea correction is correspondingly slight. Determinations obtained by Berglund, yielded : St 1467, -60; St 1468, - 60; St 1517, - 50; St 1469, - 35; St 1474, - 13. All these figures refer to material predating fallout contamination. When additional factors are taken into account, Morner's suggestion is that the correction is of the order of - 60, i.e. smaller than the standard deviation of 95% of the determinations involved in the present survey. Since the standard deviation of this correction is itself ± 90 years according to Morner, it was considered best to leave the Baltic shell dates unmodified here.

A small number of other determinations also appeared to involve standards diverging from those of the main body of information. For instance, dates referring to the isolation of some Swedish lakes from the sea are apparently influenced by the high lime content of these lakes (e.g. Olsson, 1968). Corrections for effects such as these have been suggested in some cases in the literature, but it is not clear how far these are valid. Similarly, some British workers who have found that dated peat bed samples were contaminated by younger roots have dated the roots as a basis for correcting the original date, and again the estimate is in doubt. In all such cases, the present writer considered that the overall size of the body of data available for the present survey was sufficient to make it more profitable to exclude such problematic determinations than to attempt to modify them for use with the other information. All such cases were therefore separated as Class X, and none were used in constructing the graphs of change.

In all, corrections were applied to 223 of the determinations used in this survey. These modifications are set out in detail in the Appendix listing the dates. 58 corrections were made to shell dates for the apparent age of sea-water. In a small number of these cases (identified in the Appendix) further adjustment had to be made in terms of other errors indicated in the Index. The only shell bed dates from outside the Baltic <u>not</u> adjusted by - 305 ± 25 years referred to shelly deposits in Normandy described as "Tangue". There, although Gif 390 was dated from the shells themselves (and is therefore corrected here), Gif 387, 388, 389, and 390 were in fact determined from fine organic particles among the shells. Since tangue is a strand deposit and these samples came from river mouths, it seemed best to refrain from applying seawater correction. The remaining 167 corrections were made in terms of the classes of modification given in the Radiocarbon Index. In these cases, besides compensating for changes from provisional laboratory reference standards to the internationally agreed

U.S. N.B.S. oxalic acid standard, adjustments were made for isotopic fractionation. Other changes included correction of errors due to mistakes in laboratory procedure, computation or printing. All dates involved in the present survey were checked against the Index, and without exception all adjustments specified there were implemented in full.

The aim throughout the present survey has been to obtain as reliable a perspective as feasible of the data currently available. As indicated, in order to reduce the determinations as far as possible to consistency on a technical basis, corrections have been applied to almost a quarter of the total number of dates used here. The procedure followed in making these modifications was however standardised rigorously in terms of what appears to be the best current international practice. It is important to emphasis that in the analyses which follow, the writer has refrained from making any further modification to dates, either in the text or in the calculations leading to the graphs.

It is clear that both the geological nature of the evidence and problems of sampling have made some dates earlier or later than the events to which they are related. In some cases the sense and magnitude of such discrepancies may be suspected, but at this stage so much concerning Holocene sea-level change remains in doubt that the validity of allowances proposed to compensate for differences of this kind remains difficult to assess. As in cases mentioned in a previous paragraph, it has therefore seemed preferable at this stage to avoid special arguments concerning evidence from individual sites, and to concentrate rather on establishing whether, despite such variations, the evidence contains any definite overall pattern.

iii) Reliability of the standardised C^{14} data

The initial search of laboratory lists produced just over a thousand entries apparently bearing on the survey. Some of these were eliminated when followed up in the main literature. Some, for instance, were found to refer to river deposits believed by the writers concerned to reflect sea-level change, though these proved in fact to be some distance from the coast. Only evidence intimately connected with marine changes was admitted. Other entries, though stratigraphically relevant, were abandoned because the determination of their date was classed as unsatisfactory in the Radiocarbon Index. This left just over nine hundred valid entries. Of these, a small number were of real though indirect relevance. These included, for instance, dates from the same borehole or site as dates referring directly to sea-level changes, but themselves related to earlier or later events of a different nature. Such entries were retained separately for general information and as a check on the internal consistency of local date sequences. This left some eight hundred and ninety dates of direct relevance to Holocene sea-level changes along the western seaboard of Europe.

These dates are distributed geographically as shown (Fig. 5.1). Details will be discussed later, in Chapter 11, but in broad terms it will be seen that this distribution appears to be sufficiently widespread and to cover a sufficient range of contrasting coastal conditions to suggest that it should provide a reliable indicator to help differentiate between eustatically dominated and purely local changes. Furthermore, the distribution covers the whole spectrum of Holocene land movement from Scandinavia to the Netherlands, thus embracing a wider range of land displacement than is present in Scotland. It also includes contrasting environments paralleling the range of conditions from the exposed coasts of the West of Scotland to the sheltered estuaries of Forth and Tay. It would therefore seem that the geographical distribution of the data offers an adequate basis for reliable conclusions on the lines being attempted.

The distribution of the data through time is shown in the lower diagram of Fig. 5.2. Except for four centuries (all during historic time, when the interest of radiocarbon determinations is least), relevant dates are available for all centuries in the last 10,300 years. 5 or more dates per century are available for 8,300 years in this span, 10 or more per century for 3,700 years, 15 or more for 1,300 years. Overall, most dates are available for the millennium between 4,000 and 5,000 B.P., which contains just over 15% of the total. However, However, except for the two most recent millennia of the historical period (which together have only 7%), all other millennia contain between 9% and 13% of the total (five fall in the 10% to 12% range). Basically, then, the determinations would appear to be adequately numerous and sufficiently evenly distributed for a reliable study of the full Holocene.

Fig. 5.3 shows the overall pattern of the reliability of the data in terms of the statistical uncertainty attributed by the laboratories to their physico-chemical measurements. The histogram shows the number of dates attributed to different classes of standard deviation. Generally laboratories round figures off to the nearest ± 10 years below ± 200 years, and often to the nearest ± 50 or ± 100 years above that. This procedure has been adopted in constructing the diagram. In addition, examination of the full laboratory lists suggests that some laboratories tend to round off estimates near ± 100 to that figure, rather than using the ± 90 and ± 110 steps. This tendency would appear to be reflected in the diagram. The general pattern, however, remains clear. Few dates with standard deviations smaller than ± 60 are available for the survey, but in general the laboratories attribute considerable reliability to their measurements, over 70% of the determinations being allotted standard deviations equal to or less than ± 150 years.

The distribution of variations in standard deviation through time is shown by millennia in the upper diagram of Fig. 5.2. In this, the vertical organisation is the same as in Figure 5.3, but the horizontal amplitude of the histograms has been doubled to make variations clearer. It will be noted that although the tendency is towards larger standard deviations with increasing sample age, this trend is not extreme, over 65% of the determinations prior to 8,000 B.P. still receiving standard deviations equal to or less than $\pm$ 150 years. In general, then, the statistical reliability of the laboratory work would appear to be well maintained throughout the period of interest.

This conclusion, however, leaves a major problem unresolved concerning the reliability of radiocarbon data in terms of geological deductions. As indicated above, it is now well established that fluctuations in the availability of C^{14} through time have caused perturbations in the relationship Libby originally assumed to exist between radiocarbon determinations and calendar years. Where the distortion of the radiocarbon scale relative to the calendar is a simple one, it does not interfere with the use of the radiocarbon scale as a means of relative dating. However, this is not always the case.

As Stuiver and Suess demonstrated (1966), although for each calendar year there is only one radiocarbon age, the reverse is not always true; in some instances, due to past fluctuations of C^{14} input to the biosystem, one value of radiocarbon determination may represent any of several true ages. This is particularly apparent in the last 500 years.

FIG. 5.4 & see next page.

Calendar Year	True Age	Radiocarbon Age	Calendar Year	True Age	Radiocarbon Age
A.D. 1800	150	130	A.D. 1320	630	610
1780	170	150	1300	650	650
1760	190	100	1280	670	690
1740	210	130	1260	690	710
1720	230	100	1240	710	710
1700	250	80	1220	730	730
1680	270	120	1200	750	920
1660	290	170	1180	770	910
1640	310	280	1160	790	890
1620	330	330	1140	810	880
1600	350	340	1120	830	900
1580	370	320	1100	850	920
1560	390	270	1080	870	930
1540	410	250	1060	890	950
1520	430	280	1040	910	970
1500	450	330	1020	930	990
1480	470	370	1000	950	1000
1460	490	420	250 B.C. to A.D. 1000: radiocarbon ages are generally ca. 50 to 100 years older than true ages, but deviations from this rule are possible.		
1440	510	470			
1420	530	490			
1400	550	550			
1380	570	580			
1360	590	600			
1340	610	610			

see FIG. 5.4

The implications of this shortcoming of radiocarbon as a medium for relative chronology were potentially so serious in terms of the present study that it was felt necessary to make a specific assessment of the impact of this factor on the geological reliability of the data.

Information on these variations is accumulating rapidly for the Holocene. Results based on dendrochronology have already been published, for instance by Suess (1965 and 1967); Ralph, Michael & Gruninger (1965); Damon, Long & Gray (1966); Ralph & Michael (1968); and these were supplemented by Suess, Damon, Ralph and Vogel at the Nobel Conference. Parallel work was also described there involving thermoluminescence (Aitken - cf. Aitken, Zimmerman and Fleming, 1968); varves and other rhythmites (Stuiver, Tauber); and material datable through historical chronologies (principally Säve-Söderbergh and Olsson). Further experimental work is in hand on all these fronts, and also on the theoretical determination of the variation along lines such as those explored by Bucha and Neustupny (1967).

At the Conference, Suess concluded that the main trend of the deviations can be approximated by a sinusoidal curve with a period of 10,350 years. The minor fluctuations, which give rise to the possibility of certain levels of radiocarbon determination representing any of several true dates, appeared to him also roughly sinusoidal, and they seemed to occur with a period of the order of 400 years and a total amplitude of approximately 300 years (i.e. ± 150 years) superimposed on the main trend.

Despite the progress which has been made over the last five years, it will be some time before the curve of these fluctuations is fully established and verified in detail for the entire Holocene. Both gaps and controversies still exist at present. However, the amplitude of variation considered by Suess to be characteristic of the Holocene is represented in the most recent millennium, for which Stuiver and Suess have already published a graph (illustrated above). This period therefore offers an opportunity for gauging the degree of confusion such fluctuations are likely to cause in the course of geological and archaeological deductions. Indeed Stuiver and Suess noted that the possibility of one level of radiocarbon determination being equivalent to a series of true ages was realised in a particularly marked way in the latest 500 years, so it would seem that this graph should provide a useful indication of the implications of the more complex end of the spectrum of variation believed to occur in the Holocene.

The writer has accordingly used that graph to derive an estimate of the amount of confusion likely to be caused in this way. In the following table the level of activity of C^{14} determinations is expressed in the conventional way as radiocarbon "age". For completeness, two figures for ages of less than 120 years were interpolated, and these are distinguished by brackets. Three sectors of the millennium are affected by confusion. Two of these lie within the most recent 500 years.

Radiocarbon "Age"	Corresponding True Age(s)				
60	(80)				
80	(100);	...	...	...	250.
100	120;	190;	...	230;	260.
120	140;	180;	200;	220;	270.
140	160;	180;	...	...	280.
160	...	...	...	...	290.
240	300				
260	310;	410.			

(cont'd. on next page)

Radiocarbon "Age"	Corresponding True Age(s)		
280	310;	390;	430.
300	320;	380;	440.
320	320;	370;	450.
340	330;	350;	460.
360	...	...	470.
860	740		
880	740;	...	810
900	740;	780;	830
920	750;	...	850
940	...	...	870

It will be noticed that one radiocarbon "age" may correspond to as many as 5 different true ages on the evidence of this millennium (e.g. entry 120 above). This is disturbing, but the magnitude of the variations corresponding to each level of radiocarbon determination is, however, reassuring in terms of reliability for geological purposes.

As noted above, the overall distortion of the radiocarbon scale in terms of calendar years does not affect the validity of radiocarbon determinations as a means of relative dating. Thus it is not the differences between radiocarbon "ages" and true ages that is of primary concern here, but rather the amount of confusion possible between the differing true ages that may correspond to a single radiocarbon value.

In the data listed here, the differences seldom exceed 150 years, and more than a third of the differences fall between 120 and 150 years.

The results from this millennium would thus appear to be fairly typical of the Holocene in that Suess estimates fluctuations with a <u>total</u> amplitude of 300 years (i.e. $\pm$ 150) during the last 10,000 years.

The differences found here fall well within the range of uncertainty of measurement already attributed to the determinations in the standard deviations discussed above, and indeed the average difference between corresponding true age values listed here (87 years) is of the same order as the commonest standard deviation values in the body of data as a whole.

The same is true of another source of ambiguity in the data. Although Stuiver and Suess remarked (op.cit. p.538) in the context just discussed that in contrast "for each calendar year there is only one radiocarbon age ...", their own graph shows that minor fluctuations can give rise to a situation in which the graph rises or falls so steeply that it runs virtually parallel to the radiocarbon "age" axis. The clearest example of this in the millennium illustrated falls between 800 and 900 on the radiocarbon "age" scale. It will be noticed that radiocarbon determinations falling anywhere within this range will correspond closely to the single true age of circa 740 years. In other words, in practice, not only may certain single C^{14} values correspond to several true ages, but what is within very narrow limits a single calendar age may be represented by a range of different C^{14} measurements. If the Suess estimate of the total amplitude of the secondary fluctuations is correct, however, it would seem that uncertainty arising from this effect would again tend to be of no larger order than that already expressed in standard deviation form.

In principle, then, it would seem that the individual uncertainties arising from these characteristics of radiocarbon dating should not be of such magnitude as to invalidate geological conclusions of the type under consideration here.

The possibility remains, however, that combinations of the different types of error and uncertainty might build up to unacceptable levels. The factors involved in assessing the overall reliability of the data for the present survey range from the effects just discussed, and the other inherent uncertainties of radiocarbon assay, to the limitations of the geological nature of the evidence and of field sampling procedure. A theoretical assessment of the interplay of such diverse factors seemed of dubious value. An empirical approach was therefore adopted. This was implemented on two levels, one concerned with the internal consistency of the evidence at the individual sites where the dates were obtained, the other with the nature of the overall pattern of the evidence.

Various methods of checking the internal consistency of evidence at individual sites were used in the course of building up the corpus of data. A large proportion of the dates could be checked in terms of their pollen zonation, because a very large number of the determinations had in fact been obtained by workers engaged on pollen studies. This check showed that almost all dates included in the survey were in fact compatible with their local pollen zone dates. The lengthy duration of most pollen zones made this a rather rough check, however, and the fact that the dating of the zone boundaries also relies essentially on radiocarbon work left this conclusion vulnerable even though the reliability of <u>relative</u> and not absolute dating was the subject of consideration. A more precise form of check was therefore sought.

The most positive type of check available at the level of individual sites involved the study of inversions of C^{14} date order relative to stratigraphic order, at sites where several dates were available. In cases where only one date was available from a site, and this date was claimed in the literature to be wrong, it was often impossible to be sure whether the determination or the opinion was in error. As noted above, in such cases dates have been excluded from the calculations affecting conclusions on sealevel change. Fortunately, some 630 of the dates included in the survey came from sites where series of dates are closely associated (i.e. generally obtained from the same section or bore). This represents more than 2 out of every 3 dates used in the survey. Attention was therefore concentrated on the records of these sites, to establish in which cases the order of the radiocarbon ages of samples departed from the order of superimposition of the strata from which they were drawn. This appeared to offer the most objective empirical check on the internal consistency of the radiocarbon dated data. Not only are many of the dates very closely juxtaposed stratigraphically but their spacing in time often offers a much higher degree of resolution for present purposes than that provided by the pollen zones.

It was considered that at least three different types of factors might contribute to a high incidence of inversions. Firstly, the different types of error and uncertainty inherent in C^{14} determinations might of themselves combine to produce inversions in the radiocarbon time scale. (The existence of this possibility has been acknowledged by Neustupny, 1970, inter alia.)

The second factor is the nature of the material dated. As is indicated in detail in another section, contamination by both older and younger organic material certainly occurs, and stratigraphic interfaces taken to represent transgression or regression contacts sometimes represent unsuspected unconformities. For these and other reasons it is often very difficult for the fieldworker to be sure his sampling is valid, and there could thus be no advance guarantee that such errors might not affect a large proportion of the dates gathered here.

Finally, the effect of both these factors would tend to be exacerbated if the processes causing the dated coastal changes had produced large numbers of rapid fluctuations, rather than a small number of well-spaced, decisive changes. The nature of some of the eustatic curves for the Holocene (e.g. Fairbridge 1961, Morner 1969) suggested that ocean fluctuations might be of a sufficient rapidity to interact in a complex way with the periods of uncertainty surrounding the C^{14} data, and the frequency of storm surges in the North Sea and their importance in precipitating coastal changes (Kingo Jacobsen 1964, Shepard 1965) represented an additional local factor that could not be neglected in this context.

Attention has already been drawn to the large number of dates available from sites yielding series of dates rather than single determinations. In view of the density of their distribution through time and the fact that many are derived from deposits either separated vertically by very few centimetres or actually in contact, the writer found the actual number of inversions present among the six hundred relevant dates both surprising and reassuring. What appeared to be inversions were detected at 19 sites, and directly involved 41 dates. However, it was clear that certainly one, and probably three, of these sites should be disregarded, while the inversions at three others appeared to be both geologically and statistically negligible. Significant inversions would thus appear to affect only 13 sites. These inversions directly involve 29 dates, that is only some 5% of the total number from sites yielding series susceptible to this check. That this degree of internal consistency is present at the level of the individual sites seems a positive indication that the radiocarbon dated data used here are in general reliable, despite the factors mentioned above.

The sites which it is proposed to disregard are located at Husum, Tealham and Falsterbo.

In the case of Husum (54°9°), two dates, Hv 222 and 225, in the basal peat bed there, and on the same borehole, were attributed bore depths which implied an inversion. No statement regarding an inversion was present in the text, however, and this implication of the depth figures quoted was unambiguously contradicted by statements which showed that the older date was in fact lower in the peat (Hv 225 - "25 cm below the Eesch bed") than the younger (Hv 222 - "10 cm below the Eesch bed"). It seems clear that this was merely a publication error.

In the case of Tealham, Somerset ($51^{\circ}2^{\circ}$W), Q126 5620 ± 120 was published in 1961 as a check on Q120 5413 ± 130, which had been published in 1959. Q126 was described as being taken from "just above" the location of Q120, but it is not clear whether this implies direct superposition, since no further detail is given and no comment regarding inversion is made in the entry. It is not even clear that the samples were taken on the same visit to the site. Furthermore, the 1961 determination at least was done from material kept in storage since 1955, and in view of Olsson's (1968 b) comments on the dangers of long term storage, it is considered that it is probably preferable to disregard this case also.

Falsterbo ($55^{\circ}12^{\circ}$) follows a similar pattern to Tealham. The dates involved are St 1233 7730 ± 100 and St 1216 7545 ± 100. Again sample relationships are not clearly specified, and although an inversion may be implied, the superposition is not clearly stated. The samples were collected from a temporary section in 1940 and the critical details do not appear to be known. The twenty-five year storage of the material before dating in 1965 is, in light of Olsson's strictures, a further reason for discarding this case as well.

The inversions which are considered negligible are located at Sorbackenmossen, Port Talbot and Vassbosjon.

At Sorbackenmossen ($60^{\circ}17^{\circ}$), the dated samples were in direct contact; the lower date was U 2089 4650 ± 130, the upper U 663 4660 ± 90. The The mean values are thus separated by only 10 years, which is less than 10% of the average of their standard deviations.

At Port Talbot (57°3°W), two samples on the same bore were separated by only 6 cm in depth. The lower date was Q633 8970 ± 160 and the upper Q662 8990 ± 170, i.e. a difference of only 20 years, less than 12% of the average of their standard deviations.

At Vassbosjon (58°12°), the samples were again in direct contact. The lower was Lu 220 9700 ± 111 and the upper Lu 219 9750 ± 103, i.e. a difference of 50 years, but with the means still less than half of one standard deviation apart. This sample was the result of submarine boring and in view of the sampling problems encountered it is considered best to regard this discrepancy, with the two others, as geologically negligible.

In each of the remaining fourteen cases, the minimum differences between dates are all of a hundred years or more, and most are of more than two hundred years. At certainly four and probably six of the twelve sites, the local nature of the material dated can be allotted a major role in the creation of the inversion without necessarily invoking general variations in radiocarbon input.

Thus at Saddlebow (52°0°E) the top of a peat bed is dated Q805 2415 ± 110, and 12 to 15 cm below the top the date is Q806 2275 ± 100, but the entry comments, "the whole monolith is unfortunately penetrated by vertical uncompressed rhizomes and rootlets of phragmites." It therefore seems likely that the lower sample has been contaminated in the course of this disturbance.

At Tønder, on the Danish west coast and at two submarine sites between Denmark and Sweden, erosion and redeposition rather than biological disturbance appear to have caused the inversion.

At Tønder (54°8°) wood in peat was dated K 796 3650 ± 120, but peat 5 to 20 cm lower yielded K 796 3400 ± 120. Further excavation established unambiguously that at that point the stratigraphy had been disturbed by the uplift and redeposition of older peat during a storm flood.

In the Kattegat (56^o12^o) a 10 cm sample (B 176) was secured from peat exposed on the sea bed. The top was dated St 2172 8010 ± 100, but the base yielded St 1820 7565 ± 110. The diver who obtained the sample noted that the peat is at present being eroded, and litters the sea bed in durable flakes. It may therefore be presumed that the inversion again reflected redeposition.

That this is also the case at Sjaellands Odde (56^o11^o) was confirmed by pollen analysis. There a sea bed core (B 655) showed peat layers, separated by 80 cm. The lower yielded St 2888 8930 ± 160, but the upper was over twelve hundred years older, yielding St 2289 10,195 ± 185. Pollen counts confirmed the younger date and demonstrated contamination of the other with older material.

The sites at Grasjon (59^o14^o) and Sjodyn (59^o17^o) also involved underwater sampling, but in sites that are now freshwater lakes. At Grasjon the top sample was U 269 8770 ± 190, but the other (2 - 17 cm lower) was U 218 8320 ± 140, while at Sjodyn the top was U 2046 4850 ± 140, while that below (10 cm lower) was U 2047 4580 ± 130. The possibility exists that both these substantial inversions are also due to redeposition. Considerable inversions of stratigraphy caused by systematic redeposition has been proved in other Scandinavian lakes, notably in a comprehensively dated study at Kolundakarret (Uppsala V, Radiocarbon 7, 1965). In view of Olsson's warnings against long storage of materials before dating (op.cit.) it may also be noted that in each of these cases the samples were stored for over five years. These two sites are further considered below, along with the others for which local conditions offer no immediate explanation.

The site at Mesnil in France (49^o0^o) falls into the latter category. It is set apart from the other inversion sites, however, by the magnitude of the uncertainties surrounding the laboratory measurements. The standard deviations attributed to the dates of most of the other sites are less than ± 150, and none are as large as ± 200. In the case of Mesnil, however, the quoted uncertainty is twice as large as at most of the other sites. Thus even though the inversion represented by the three dates extends for some 530 years overall, the mean of each date is within one standard deviation of the next in time, and the extremes fall within 5 years of being within two standard deviations:

e.g. depth	9.9 - 10.5 m	Sa 69 7630 ± 350
	11.1 - 11.5 m	Sa 70 7380 ± 350
	11.9 - 12.2 m	Sa 71 7100 ± 350

Three dates are also involved in the inversion at Silverdale Moss, Lancashire (54°2°W) although in this case, in contrast to Mesnil, the double inversion rests on one date (Q 256), in that the other two dates involved do not of themselves represent a disagreement with stratigraphic order.

The top date was: Q 261 5865 ± 115

The middle date was: Q 260 6590 ± 114

The lowest date was: Q 256 5734 ± 129

Olsson's warning about storage must again be cited, since although the determinations date from 1961, the samples were secured from a monolith which had been stored in the laboratory since 1957. The samples at Silverdale were spread over a vertical range of the order of half a metre.

At Hulebo (57°12°) the double inversion again rests on only one date, St 1531.

The top date was: St 1531 7730 ± 165

The middle date was: St 1532 7545 ± 150

The lowest date was: St 1533 7585 ± 190

Unlike Silverdale, at Hulebo the extreme dates lie within less than one standard deviation of each other. The samples are again spread over a little more than half a metre vertically.

At Torekov ($56^{o}12^{o}$) only two dates are involved.

The top date was: St 2336 5025 ± 100

The lower date was: St 2286 4890 ± 100

i.e. a difference of 135 years, or less than 1.5 standard deviations. The samples were separated vertically by 40 cm.

At the remaining three sites the pairs of samples were in contact. The dates involved are :

Mellosa ($56^{o}16^{o}$) top: U 1016 7750 ± 70

lower: U 488 7650 ± 120

i.e. a difference of 100 years with mean value of U 1016 falling well within + 1 standard deviation of U 488.

Brakemotet ($57^{o}11^{o}$) top: Lu 21 9060 ± 105

lower: Lu 22 8860 ± 100

I.e. difference 200 years, means within 2 standard deviations.

Torp ($57^{o}12^{o}$) top: St 1529 8980 ± 170

lower: St 1530 8620 ± 170

i.e. difference 360 years, 20 years more than 2 standard deviations between the dates.

No sure local explanation (such as direct proof of contamination or other disturbance of deposits) appears to be available for the inversions at Mesnil, Silverdale, Hulebo, Torekov, Mellosa, Brakemotet or Torp, nor for Grasjon or Sjodyn. If the dates from these sites are examined, it will be found that with the exception of Silverdale, Torekov and Sjodyn, the dates appear to fall into two groups in time, one including Torp, Brakemotet and Grasjon, the other Mesnil, Hulebo and Mellosa. It is interesting that two of the less secure cases considered above (Port Talbot, 20 year difference only, and Falsterbo, superposition not proven) also happen to fall in these groups.

Inversion Group I	Inversion Group II
9060 Brakemotet	7750 Mellosa
(8990 Port Talbot)	7730 Hulebo
8980 Torp	(7730 Falsterbo)
(8970 Port Talbot)	7650 Mellosa
8860 Brakemotet	7630 Mesnil
8770 Grasjon	7585 Hulebo
8620 Torp	7545 Hulebo
8320 Grasjon	(7545 Falsterbo)
	7380 Mesnil
	7100 Mesnil

The number of sites involved (four in each case) is too small for any definite conclusions to be drawn, but there would seem to be some possibility that this grouping of dates representing otherwise unexplained inversions might reflect effects of the type discussed above in terms of the Stuiver and Suess graph. Unfortunately, this can not yet be checked independently in terms of the dendrochronological calibration of the C^{14} fluctuations, because the earliest definitive record so far available for this ends at 7100 B.P. (Ferguson, in Neustupny 1970).

A recent note on bristlecone pine work by Suess (1970) places radiocarbon fluctuations of this type at four periods between 7000 B.P. and the present. The most recent of these has already been considered in some detail, above. The earliest perturbation occurs about 5550 B.P., then there is another around 4950 B.P., followed by several in the period between 4450 and 3750 B.P. On the information available at present, the confusion at Silverdale seems to occur somewhat too early to be attributed to the first of these. As noted above, it is not clear whether an inversion in fact occurred at Tealham. It is interesting, however, that the dates there exactly straddle the perturbation centred at 5550 B.P. (5620, 5412). The definite inversion at Torekov also coincides with the perturbation at 4950 B.P. (5025, 4890). No inversions appear attributable to the fluctuations between 4450 and 3750 B.P.

It seems clear that within the next few years it will become possible to make detailed allowances throughout the Holocene for the effects of these radiocarbon variations. This will in turn make it easier to distinguish between different kinds of dating error. For the moment, however, the first priority remains the diagnosis of the overall importance of the combination of all types of error. The way that 95% of the radiocarbon dates from sites represented by series of dates conform to their stratigraphic order would appear to suggest strongly that the reliability of the evidence included in the present survey is high. The validity of this conclusion may be tested by a different type of approach to the data.

As suggested above, the reliability of the available dates may be considered not only at the level of consistency at individual sites, but also in terms of the overall pattern of the evidence. The form of statistic used to describe the distribution of the dating evidence through time is discussed in some detail below. In the present context it will serve to note that the distribution is described graphically by what is in essence a form of histogram, refined to take into account the probability distribution of the individual dates (as defined by their laboratory standard deviations). The system of classification used for the data is set out below. Some of the information is geologically ambiguous, but the majority of the dates fall clearly into stratigraphic categories which indicate either transgression or regression of the sea at the sites dated.

Other things being equal, with a fluctuating sea level the curves representing the amounts of transgression and regression evidence could be expected to have an inverse relationship, i.e. during a rise of sea level the curve of transgression evidence would tend towards a maximum, while the curve of regression evidence tended towards a minimum. In the form of display used in the main graphs of the present work, the curve of regression evidence has accordingly been shown inverted, beneath the corresponding transgression curve, in order to prevent visual confusion.

Thus, in an ideally simple situation in which movement of the sea's surface was the only factor, the graphs could be expected to take the form shown in Fig. 5.5A.

If the curves instead showed no clear pattern but were characterised instead by unrelated fluctuations (Fig. 5.5B, say), one possible explanation would be that the influence of eustatic fluctuations had been outweighed by purely local factors (e.g. local variations in land movement), and local environmental changes such as the creation and breaching of protective sand barriers, etc. Alternatively, the various types of uncertainty and error inherent in radiocarbon dating could have combined with imperfections in field sampling to reduce the fidelity of the dating below the level of resolution required by the prevailing rate of the sea level changes. In practice, a graph of this nature (Fig. 5.5B) would probably reflect a complex interplay of both these types of factor.

If, on the other hand, the curves tended towards the form illustrated in Fig. 5.5C, it would seem likely that the imprint of eustatic changes had been so weakened by factors of the kinds indicated above that radiocarbon fluctuations of the types described by Suess had emerged as the dominant element in the pattern of the evidence.

In the interests of clarity, the three diagrams used here to illustrate these points are extreme in form. It was expected that in practice the form of the curves might well be such as to render interpretation difficult. When the actual curves for the data of the survey were derived (as detailed below), the writer was therefore surprised to find that they showed a clear pattern (viz. Chapter 6), with an unambiguous alternation of peaks of transgression and regression evidence that was closely analogous to Fig. 5.5A.

As will be shown, considerable care was taken to exclude subjective judgements from the classification of the data used (the distinction between "transgression" and "regression" evidence was made on strict stratigraphic grounds). The two curves were plotted separately, their form being defined by calculations made for stations separated by intervals of no more than 1% of the abscissa throughout the length of the graph. (Calculations were in fact made at ½% intervals in the areas of the maxima and minima to check their definition.) It is thus considered that confidence may be felt in the reality of the relationship between the two curves.

It would thus seem that the overall pattern of the evidence gives good grounds for concluding that the data are dominated neither by the shortcomings of field sampling nor by the limitations inherent in radiocarbon dating, and that the reliability of the evidence is in fact adequate for the study in hand.

The further implication of the pattern, that general changes of sea level have dominated the pattern of local coastal changes, is reserved for fuller discussion at a later stage, as is the assessment of detailed variations in reliability. At present it will suffice to note the clarity with which the alternation of peaks of transgressive and regressive evidence emerges from the data.

In conclusion, then, it is accepted that radiocarbon ages follow a scale which is distorted in terms of true calendar ages. The concern here is with the reliability of the relative dating of the samples included in the present survey. At the level of individual sites, both the consistency of radiocarbon dates with pollen zonation and the internal consistency of stratigraphically superimposed series of radiocarbon dates is considered encouraging. Although some 630 dates (well over two-thirds of those available for the survey as a whole) were subject to the latter check, the order through time of 95% of these conformed to their stratigraphic order and inconsistencies of more than 200 years (from all causes) were only apparent at eight sites in all. At the level of the overall pattern of the data, the nature and clarity of the relationship between the distributions of transgressive and regressive evidence through time was equally reassuring. Errors and uncertainties of the various types described are certainly present, but both levels of enquiry suggest that the general reliability of the data of the survey is adequate for the type of investigation pursued here.

The large number of relevant dates which it has proved possible to assemble here compares favourably with the number used in previous studies of Holocene sea level changes.

For instance, the most widely quoted and discussed study of the present decade is that presented by Fairbridge in his major paper of 1961. His graph of eustatic changes is widely quoted and indeed reproduced in many recent papers dealing with changes in the coasts of western Europe (ranging from Müller, 1962, to Jelgersma, 1966, Köster, 1968, and Morner, 1969). However, at the time he prepared that paper, the number of radiocarbon dates available concerning sea level change was very much smaller than it is now. He in fact commented specifically on the paucity of relevant radiocarbon data then prevailing, even on a world scale. His graph thus had to depend primarily on only some seventy dates, i.e. about 8% of the number available for the present survey. Furthermore, in terms of the current standards (as set out in the Journal Radiocarbon and applied in the present study - see above), about thirty of Fairbridge's seventy dates must either be abandoned as unreliable, or modified.

Similarly, Jelgersma's graph of changes in Holland is still widely quoted and reproduced as published in 1961, even though 36 out of 52 dates used there have since been altered by amounts ranging up to 340 years in the course of laboratory recalibration.

It is hoped therefore that both the overall number of the dates used here, and the fact that such corrections have been applied throughout, will secure more reliable conclusions than have been possible in the past.

iv) The classification of the evidence

The use of punched cards and Xerox copies has already been mentioned. This allowed rapid access to either the full picture at any single site, or to all sites within any geographical area. Alternatively, all dates relevant to a given period, or periods, could be extracted. It also made it possible to sort out quickly from the corpus all entries referring to any particular characteristic of the data. As indicated earlier, this might range from strata thickness or altitude to the availability of pollen or diatom data. Routine classification work of characteristics of this type could be performed mechanically, but the classification of the data in geological terms required careful consideration. Not only would it affect the objectivity of the survey, but some superficially logical ways of ordering the data might later prove to have inherent shortcomings, and impose unforeseen limitations on the productivity of the work as a whole.

One possibility was classification of the dates in terms of the geological features to which they had been attributed in the literature, such as the Scandinavian "Litorina" and "Tapes" systems of shorelines, or the Netherlands sequence of buried "Tidal Flat" deposits. Then the equivalents might be worked out (for instance, from the dates it seems that some Litorina I sites of Finland correspond closely to Tapes I in Norway and to the oldest major Tidal Flat deposit in Holland).

It became clear however from the literature that these names are not always used in a consistent way. This will be illustrated in some detail below with regard to the well-known "Ancylus" and "Litorina" stages of the Baltic. The variability of usage apparent in these cases seems characteristic. Furthermore, errors of attribution were often admitted in the date lists, while in other cases such errors are to be suspected. The possibility also exists that some of the conceptual models implied by these traditional terms may not be wholly valid. Since the present study is essentially an attempt to contribute towards a sounder model of Holocene sea level change than that represented by the terms long traditional in Scotland, it was clearly preferable to seek an alternative form of classification that would avoid the preconceptions inherent in such terms as "Duinkerke I and II", "Schwabstede" or "Tapes".

The problems of interpreting the heights of old marine features and deposits have already been indicated. Uncertainties of measurement and of local compaction since deposition are combined with uncertainties regarding the original relationship of deposits to their contemporary mean sea level. These difficulties place serious limitations on the value of attempts to use reported heights as the basis for correlating old marine features, particularly in areas such as that under study, where the pattern of differential land movements through time has by no means been fully elucidated in quantitative terms. Thus, although certain secondary conclusions seem possible on the basis of the available height data, height was rejected as the primary criterion for the organisation of the investigation.

It was concluded that stratigraphy offered the best basis for the objective classification system. What was sought was the least complicated form of classification that could yet avoid any oversimplification or loss of data. Because of the amount of information involved in the appraisal of some thousand dates, it proved necessary to carry out extensive experiments with several types of scheme before their shortcomings could be properly assessed.

For instance, with some two-thirds of the dates in the survey drawn from sites represented by series of dates rather than single determinations, it seemed well worth considering a coding system based directly on the combinations of stratigraphic units apparent at the different sites. This approach has been used most effectively in mapping Holocene deposits, particularly in Holland (e.g. Verhandelingen Koninklijk Nederlands Geologisch-Mijnbouwkundig Genootschap, Geologische Serie 21, 1963).

However, the way that the radiocarbon dating samples were related to the stratigraphic units was extremely variable. For instance, at a few sites, either the tops or the bottoms of intermarine peat beds were consistently used as the source of samples. Characteristically however the fieldworkers drew their samples in an irregular fashion (e.g. mixing dates from tops, bottoms and midpoints of peat beds in a single section). The expense of C^{14} determinations made it necessary for them to be highly selective, and the achievement of a formal consistency in sampling the combinations of strata under study was usually prevented by the necessity of solving the maximum of local problems with the minimum of radiocarbon dates. It became apparent that the elegance of classification possible in terms of combinations of strata (i.e. instead of individual stratigraphic units), was in practice lost in the complexity of the secondary classification system required to define the irregular placing of the dates at each site within a given combination category.

An alternative system was then sought in terms of individual stratigraphic units and their immediate significance in terms of local marine transgression or regression. In part this worked well, and aspects of it have been retained in the system which was ultimately evolved through these studies. For instance, many dates are available from the east coast of the North Sea which refer specifically to basal peat beds (i.e. Holocene beds resting directly on Pleistocene material), which reflect rises of water table, and which are directly overlaid by marine clays. If such dates are classed together, it is likely that the analysis of patterns within that class will be informative. In other cases, however, a classification with so specific implications is not possible, at least with equivalent objectivity.

During the pilot study this became particularly apparent with regard to shell dates and certain other peat dates. Sea shells have often been dated as indicators of coastal change and sometimes even in the Holocene their occurrence far from present sea level can provide dramatic evidence of land and sea movement, (e.g. T 119c 9395 ± 275, Mytilus edulis from shell bed near Oslo at 201 m above present sea level - cf. also T 180 - 201 m; T 291 - 148 m; T 287 100 m inter alia). Such gross displacements are however exceptional in the corpus of evidence considered here. Both the relatively small amplitude and, more particularly, the rapidity generally characteristic of Holocene marine changes often makes it necessary to attempt to distinguish specifically whether shell beds located near present sea level represent conditions of marine regression or transgression. Because of the subtlety of the Holocene changes, the mere existence of an ancient shell bed and its date are of limited value in a study such as this.

In some cases, dated shells are described as representing a regression or transgression but in few cases (discussed in detail below) is this more than an assumption. The present writer has worked for some years with the marine biological research team led by Dr. Clifford Johnston and Dr. John Duffus (e.g. Johnston, Morrison and Maclauchlan 1969). In the course of work on extensive ecological programmes (involving specialist macological studies), he has become impressed by the extent of the difficulties and ambiguities involved in deciding whether shell beds relate to regressive or transgressive phases. On detailed examination of the literature relating to shell sites figuring in the present survey, it became apparent that any attempt at a general classification of the available shell dates on these grounds would be unprofitable, and unreliably subjective. The problem of interpretation is exacerbated because some shell beds contain a mixture of shells of distinct age groups. Although this is sometimes obvious (e.g. when, say, rolled examples of Arca glacialis occur amongst well-preserved shells reflecting warmer conditions), this is by no means always the case.

Ambiguities also hamper the effective classification of some dates from peat beds. Some workers for instance fail to specify where they took their samples in quite thick intermarine peat beds. It is thus not clear whether the date is best considered as representing the start of a regression, some indeterminate point within it, or the onset of the succeeding transgression. Similarly, thin layers of peat interrupting clay deposition may represent the initial colonisation of that surface (if, say, any later growth was eroded in the succeeding transgression) or, alternatively, it may be that the resumption of the transgression sealed off from decay the last vegetation to have grown on that surface, earlier vegetation having failed to survive in subaerial conditions (if indeed it was ever present). Even when pollen analyses show that the dated layer represents saltmarsh conditions, it is not always clear whether this indicates the start or end of a regression or transgression, or perhaps the only representation in a marginal area of the maximum of a major recession, represented contemporaneously elsewhere by fully terrestrial forest conditions in other intermarine peat beds.

Because of difficulties such as these, the classification which was finally evolved incorporated first of all a division of the data in terms of the reliability in geological terms with which conclusions might be drawn from it.

As has already been indicated, determinations classed as unreliable in the Radiocarbon Checklist were altogether eliminated from the survey. Other dates requiring problematic adjustments were classed X, and admitted provisionally in the category of lowest geological reliability. Although for completeness these dates are included in the corpus, it in fact proved possible to avoid making any use of them in the analyses. The exclusions are identified in detail in Chapters 8, 9 and 10.

The next level of reliability comprised those dates which were fully acceptable chronometrically but which on geological grounds it seemed preferable not to include as prime indicators of marine change. This level included all shell dates (class S) and those peat beds of the problematic types discussed in the previous paragraph (class P), together with underwater forest beds (class U).

This last class contains submerged forest beds which are not effectively buried but instead appear underwater on the present sea bed. Many of the peat and forest beds occurring below present sea level in Europe are well sealed by superimposed deposits, and are thus considered valid as primary evidence, as will be shown below. The exposed beds too exhibit a consistent grouping in time, and this conforms closely to major events, as defined by other lines of evidence. Nevertheless, their exposure renders them liable to interference, both natural and human.

The existence of these beds indeed usually comes to the notice of geologists through fishermen who have brought peat and treestumps up in their trawls. The fishermen dump the debris when they finally clear their net, and this may be anything up to six hours steaming after the material entered the trawl. The shallow coastal waters where the exposed forest beds are found have now been trawled for a very long time. The trawl appears to have been known in Roman times, and as long ago as the 14th century over-trawling was already being considered **damaging** (**March,** 1953). Serious disturbance thus seems likely. Trawlermen with whom the writer has discussed this pointed out that it is not uncommon to dump such trawled debris systematically in unfrequented areas crossed while changing grounds. That this has in fact happened to some of the material included in class U is suggested by the grossly anomalous depths from which it was recovered. Thus even though the dates of treestumps involved conform exactly to the date ranges of the transgressions which killed other submerged forest material still apparently in a position of growth, it was considered preferable to include all class U material with classes S and P.

A further class was also regarded as falling at this level of secondary geological reliability. This "miscellaneous" class (M) contains dates referring to unusual circumstances that did not coincide with any of the other classes used. Many of these contain elements of ambiguity which render their relegation to this level of reliability inevitable. Others, however, offer clear evidence equal to that required for inclusion at the top level of the scheme. It was, however, considered that though these individual dates were compatible in geological terms with that level, the fact that they were from rare circumstances made them less reliable than the standard desired for the primary evidence, in that their presumed relationship to other classes of evidence could not be checked empirically. Thus, individual dates might appear to agree with the patterns apparent in other classes of data, but such an appearance in a rare and unrelated case was considered less reliable than correspondences between classes each made up of large numbers of dates. These cases too were therefore segregated from the primary data.

With the removal of classes X, S, P, U and M, it was considered that all the remaining dates could be rated at the top level of geological value. Not only did they all fall into unambiguous stratigraphic categories relating directly to local transgression or regression, but they also represented classes which each contained enough dates to yield an independent pattern of evidence covering the greater part of the Holocene.

At this level, four classes of date could be distinguished. These were labelled T,R, W and I. The T. and R classes contain respectively the dates of transgression and regression contacts in stratigraphy. Class T thus refers to dates from the <u>top</u> of peat or forest beds where they are immediately overlain by a marine deposit (usually clay) and class R correspondingly to the <u>bottom</u> of such a bed where it overlies a marine deposit. Where pollen or diatom studies were available these were used to check the validity of the date at the contact in terms of local marine change. The "pitchy" soil dated where possible by Swedish workers such as Morner (1969) represents the vegetation on a transgressed ground surface, sealed and preserved by the immediate deposition of marine sediment. It thus also represents a particularly reliable type of transgression contact date.

In all cases where pollen or diatom evidence suggested an unconformity, the date was relegated to class P in the secondary level. In many cases, however, none of these checks were available, and though classes T and R thus objectively reflect the stratigraphic location of the sample dated in descriptive terms, they do not guarantee that there is no unconformity. Unconformity may for instance exist in class R because local conditions did not initially favour the growth or the preservation of peat when first the sea regressed. It may also occur in class T, for instance, because bog senescence inhibited growth sometime before the onset of transgression, or because in the course of the transgression the top layers of the peat were stripped away by wave action before permanent sedimentation commenced.

In the case of individual dates it is often impossible to eliminate possibilities such as these. Except in cases where pollen or diatom evidence was specific, this was not attempted, because the aim throughout the present survey is to achieve an objective inventory and perspective of the data currently available, and to eschew the type of judgements which might merely reflect preconceptions regarding the pattern of change. Errors in the dating due to unconformities thus certainly contribute to the overall pattern of dating uncertainties. Although it is not yet possible to distinguish individual errors, as will be shown below an indication may be obtained of the general level of uncertainty and its variation through time. It was considered preferable to show this as objectively as possible.

The remaining classes are W and I. W, standing for "Watertable", refers to the basal peat overlain by marine clay which has already been mentioned. As noted there, it is most reported from the eastern shores of the North Sea, where characteristically it lies unconformably on Pleistocene sands. The most detailed study of this type of peat has been by Saskia Jelgersma, and in 1961 she showed convincingly that its growth and subsequent preservation was due to the rise of watertable caused by sea level rise reaching its level. The specific relationship of this evidence to that of Class T will be examined in detail below, as will the relationship between classes R and I. Class I refers to the isolation of shallow arms from the sea and their conversion into freshwater lakes. The process was not always a simple one, and in Scandinavia some present day lakes show evidence of having been isolated from and reconnected to the sea as many as five times in the course of the interplay between changing land and sea levels (e.g. Lake Kolbengtserodssjon, documented as Site 51 in Chapter 8.) With changes in sedimentation of this complexity, much of the evidence in class I rests on radiocarbon dating of changes in diatom population, but grosser variations are often also apparent.

Nine classes have thus been used: X, S, P, U, M, T, R, W and I. Once the principles of this classification had been evolved as set out here, provisional classifications were abandoned and the classification of all dates was reassessed afresh. Direct reference was again made to the full Radiocarbon entries (held as Xerox copies on the punched cards), and these were supplemented extensively from the relevant journals and monographs. In a survey of this magnitude it is not practical to indicate the specific references consulted in the course of the classification of almost a thousand dates, but a representative bibliography is given with the Appendix in which the corpus is set out.

The task of classification inevitably took some time, so it was considered advisable to check that standards had not changed during the course of the work. The first full classification had been carried out site by site, with the sites grouped under countries for convenience in dealing with the literature. For the check, this order was broken and the material was organised under its classes, P, T, R, etc. Consistency within each class was then examined.

In both the main classification and the check, strictness was observed in admitting dates to the first level rating of T, R, W and I. When any doubt about stratigraphic integrity or interpretation seemed present, the date concerned was relegated to the second level. In this process, most rejects from T, R and W went into P, most from I into M. Such relegation could be carried out without compunction, since it did not involve any loss of information from the survey as a whole, merely the making of a clear distinction between information which required further discussion, and more clearcut data which might offer a starting point for that discussion.

When classification work was complete, it was found that, omitting Scotland, 435 out of the total number of 860 dates available from the western seaboard of Europe fell within the four groups which could be classed geologically as prime indicators of changes in relative sea level. That such a large number could be classed in this way was considered encouraging, firstly because it represented a rather higher proportion of the total (ca. 1 in 2) than originally anticipated, and secondly because this number alone was considerably larger than the total number used in most previous studies.

The work of classification was completed in its entirety before the work of deriving the graphs representing the initial hypothesis of change was put in hand. This was done deliberately, so that the classification of the dates reflected only their immediate stratigraphic context and the fullness with which this had been investigated by the fieldworkers. For reasons stated above, no account was taken in the classification work of published attributions of sites to concepts such as "Litorina I" or "Duinkerke II" and, in a similar effort to avoid personal expectations influencing the work, the writer allowed himself no changes in classification once the calculations for the graphs were started.

Some writers on sea level change have sought to achieve a clearer picture by modifying their data selectively. Though in some contexts this may be acceptable (the example of Fairbridge's important paper of 1961 will be examined below), it seemed desirable to maintain a clear distinction here between on the one hand the type of "on site" criteria used in making the classification and on the other, the type of consideration arising from the overall pattern exhibited by the data through time. Thus, although it would have been possible to optomise the graphs derived below from data classed at the top level of geological reliability by reclassifying as "doubtful" those dates which departed from the dominant pattern, it was considered both sounder and more informative to leave the classification as it stood throughout, and instead use an additional procedure to obtain a measure of the inconsistency produced in the graphs when this classification was uniformly and strictly applied.

v) The approach to the statistical handling of the evidence

That some form of systematic statistical treatment of the radiocarbon dates was in fact a necessity, if the conclusions drawn regarding sea level changes were to be self-consistent, became particularly apparent to the writer in the course of a detailed examination of Fairbridge's major paper of 1961.

It has already been noted that the extent to which the graph of eustatic change presented by Fairbridge has been quoted in the literature marks this as one of the most important papers on sea level change published in this decade. The key curve in his graph passes smoothly through small dots representing the radiocarbon dates that formed his data. These are labelled with their laboratory numbers, and since he does not clearly indicate otherwise, the reader's presumption is that these dots represent the nominal values of the dates (i.e. the published means of the laboratory determinations). The present writer checked this and found that the positions of some dots did not correspond to these values, but that they had been displaced in order to secure a continuous curve.

The amounts of his individual adjustments seemed legitimate in terms of the statistical uncertainties quoted by the laboratories (according to the present writer's checks; Fairbridge himself nowhere lists either the mean values or the statistical errors of the dates). However, even if all the adjustments were individually within reasonable limits, he may be criticised for inconsistency in that he was highly selective in his use of the statistical uncertainties surrounding the dates. If each date is represented not by a dot but by a line parallel to the abscissa with length proportional to the standard deviation of the date then (even without taking into account the uncertainties surrounding the height determinations he uses) it seems that smaller undulations of his curve may lie beyond the limits of resolution of his data.

Despite criticisms such as these, Fairbridge's attempt to produce coherence from the limited amount of difficult data available in 1961 clearly merits the attention it has received.

His aims and material differ, however, from those of the present writer. Whereas Fairbridge sought to stimulate interest in eustatic studies on the basis of the small number of relevant dates then available, the writer is concerned with extracting as reliable a set of control information as possible from a considerably larger number of dates. It therefore seemed particularly necessary to devise a statistical approach which would allow the data to be explored in a more consistent way.

As a starting point, it was therefore decided to seek an approach which would yield objective descriptions of the patterns through time of the different classes of data (i.e. T, R, I,W, etc.) with due allowance for the statistical uncertainties known to surround the laboratory determination of the dates. If this could be achieved, some indication of the overall effect of the various other chronometric and geological sources of error and uncertainty (discussed above) might then be sought from an investigation of inconsistencies between the patterns exhibited by the geologically related classes of dates.

Despite the large number of recent articles on sea level change in which radiocarbon dates figure prominently, it proved surprisingly difficult to find any systematic statistical approach to the description of date patterns which might serve as a model for the present survey. Even though both the means and the standard deviations of radiocarbon determinations are usually now quoted in literature on sea level change, the time patterns of the determinations are usually discussed only in verbal terms, not quantitatively. When radiocarbon dates figure in diagrams, they are usually represented by points or bars showing the range of the determination to one (or occasionally two) standard deviation limits from the mean. Published curves constructed on the basis of radiocarbon dates almost invariably appear to have been interpolated by eye. In the context concerned, this is clearly acceptable. However, it made it necessary to look beyond the literature of sea level change to find a technique suitable for present purposes.

The commoner statistical methods did not appear to offer a ready-made approach. Most types of time series analysis are concerned with the extraction of overall trends or cyclical components, and were thus not of primary interest here, while the basic uncertainties regarding firstly the nature and secondly the general level of complexity of Holocene coastal changes in western Europe suggested that the use of most techniques of statistical correlation would be complex, and would involve considerable problems in interpretation. As Miller and Kahn note in their comprehensive work on "Statistical Analysis in the Geological Sciences" (1962),"it appears that the application of time series analysis to geological problems is essentially an untapped field of investigation", and investigation of the correlation between pairs of time series in geology a notably neglected part of that field (p.363).

Since well established approaches were not readily available among purely numerical techniques, the writer turned to the consideration of graphical methods. In a series of publications, Professor G.H. Fisher and his team have made a strong case for the value in research of visual approaches to statistics, particularly those involving the standard normal distribution function (1965; 1966; 1967 a and b; 1968). They showed in a number of studies that research workers who were not professional statisticians made fewer errors when working through visual presentations of data. It was therefore decided to attempt to develop a form of statistically reliable visual description of the patterns of the various classes of evidence through time, with the aim of producing a display that might be considered analogous in general characteristics to a pollen or diatom diagram, i.e. variations through time within each class should be portrayed objectively and with numerical accuracy,and major relationships between the patterns of variations in different classes should be accessible on inspection.

The most familiar types of pollen and diatom diagrams show variations in the frequency of occurrence of pollen or diatoms of different species or species-groups in a series of sample counts, made at closely-spaced intervals through the deposit of interest. The order of superposition of samples is taken to represent their order through time, although because of variations in rate of deposition, compaction, etc., age and depth in the deposit seldom have a simple relationship. However, unless special factors interfere, the pollen counts for different species may be assumed to refer to the same period in time if drawn from the same point in depth in the core sampled.

The situation is thus closely analogous to that encountered with radiocarbon dates in the present study, in that although radiocarbon "age" (like stratigraphic depth in the pollen core) does not bear a simple relationship to true age, as discussed above, it appears to be at least generally valid to compare the incidence of different geological classes of determinations (T, R, etc.) on the assumption that points on the pattern of each class bearing the same radiocarbon date refer to the same period of time.

In pollen and diatom diagrams the patterns of species-change through time are usually displayed either as histograms or continuous curves. In the case of radiocarbon date diagrams, one obvious possibility was the use of a simple histogram or curve representing the number of dates in each class whose mean values fell within a radiocarbon age interval of given size, say a century. This approach was used above (Fig. 5.2), but while it was considered adequate for an initial description of the broad time distribution of the data, a more refined approach seemed advisable for the main part of the investigation.

In view of the cyclical nature of some of the mechanisms which appear to influence fluctuations in apparent radiocarbon age (e.g. Neustupny 1970 quotes a variety of examples currently under investigation), it seemed possible that the construction of histograms or other diagrams on the basis of counts of dates falling within given time intervals might give rise to complex interference effects if the intervals selected interacted with important cycles or their harmonics. The likelihood of this is difficult to evaluate on present knowledge (Neustupny, op.cit.). It therefore seemed preferable to avoid incurring possible additional complexity from such interference effects by adopting a form of representation in which dates were not grouped in arbitrary divisions but were plotted in terms of the actual mean values of their determinations.

Furthermore, it seemed desirable to take as full account as possible of the varying statistical uncertainties attributed by the laboratories to their measurement of the individual dates, and this again made a continuous form of representation seem preferable to one involving grouping in arbitrary age divisions.

Several methods of allowing for these laboratory uncertainties were considered. The one finally adopted involved a very direct recourse to basic statistical principles. The laboratories quote determinations in terms of a mean value, plus or minus one standard deviation. Uncertainty is inherent in the measurement, because it involves the counting of a sample of radioactive emissions and these occur randomly. The standard deviation is generally recognised as the soundest indication of scatter in mathematical terms (Gregory, 1963). As is well-known, it is a descriptive measure used for probabilities which can be assumed to be distributed according to the standard normal distribution function, which is of the form of a Gaussian curve.

The equation of this curve (quoted below) is such that with a knowledge of the mean and standard deviation every other characteristic of the overall probability distribution may be determined in terms of the distribution of the area under that curve. Moreover, the measurement of standard deviations from the mean establishes positions between and beyond which known proportions of the total frequency lie. For instance, the area under the curve partitioned off by the values corresponding to the mean plus and minus one standard deviation is equivalent to a range of the variable comprising approximately 68% of all the frequencies, while only about 5% of the distribution lies outside the range corresponding to the mean plus and minus two standard deviations. It will be noticed that a small standard deviation thus implies a tall narrow Gaussian curve, with the probability that the measurement lies close to the mean, whereas a large standard deviation implies a broad distribution with a wide range (Fig. 5.6, from Yeomans V2, p.32, 1968).

In principle, it seemed reasonable to the writer to describe the radiocarbon data graphically by means of Gaussian curves proportioned in terms of their individual standard deviations, and positioned along an axis representing radiocarbon age in terms of the mean values of their determinations. It was clear however that because of the large number of dates involved, diagrams of this form would be very complex visually (Fig. 5.7). An additional step was clearly required to derive an objective expression of the pattern of this data in visually simpler terms.

Considerable effort had been made at laboratory level to ensure that all dates were of a common standard chronometrically, and (as described above) the present writer had ensured that the latest available results of laboratory cross calibrations and other corrections were applied to all dates included in the present survey. Furthermore, it will be recalled that a primary objective of the writer's classification of data was that all dates in the top level of classification (categories T,R,W,I) should represent a uniformly high level of geological validity. It was therefore considered that the additional step of statistical procedure might legitimately start from the assumption that all dates, at least in the key classes T,R,W and I, could initially be considered of equal validity.

The possibility thus arose of obtaining an overall indication of the pattern of evidence in each of these classes on the basis of that assumption, by : A) allotting an equal area on the graph to each such date;

B) disposing this area around the mean value of that date according to the probability distribution defined by the particular form of Gaussian curve specified by the laboratory standard deviation of that date;

and (C) summing the ordinates of all such curves to derive a single curve that would serve as an objective expression of the overall distribution through time of the individual probabilities.

Pilot experiments suggested that patterns produced in this way were not meaningless in geological terms. A search was therefore undertaken in the statistical literature to find whether any precedent existed for this approach to the handling of data expressed in terms of the normal distribution function. A paper delivered to the Royal Statistical Society by Thom in 1955 was based on graphs derived exactly according to the procedure described above, and in the ensuing discussion (J. Roy Statist.Soc. A 118 1955, p.291), Professor M.G. Kendall commented particularly that "his diagrams seem to me very convincing." This seemed encouraging. Variants of the same technique figured in other studies by Thom (1961; 1967). Professor Thom has very generously examined an account of the chronometric and geological nature of the present writer's material, the type of inference it was hoped to make from it, and the way in which it was proposed to employ his form of graph to help secure this. He confirmed that this application of the technique was valid.

In interpreting pollen and diatom diagrams, most emphasis is usually placed on the general pattern of maxima and minima in the curve for each species (or species group), and on the relationships between the peaks in different groups. In general, although the <u>relative</u> size of different peaks in the graphs is considered with interest, caution is shown in attempts to drawn detailed conclusions from the <u>absolute</u> size of individual peaks, because this is influenced by so many diverse factors (e.g. conditions of original deposition, subsequent preservation, collection, laboratory pre-treatment, limitations of sample count, etc.)

The chronometric and geological uncertainties involved in the present study suggested that similar caution would be advisable here, and accordingly no conclusions have been based on measurements of the absolute magnitude of peaks in the graphs which follow. This conforms to Professor Thom's own practice up to this time. He devised the technique essentially to show up the basic patterns in difficult data, and in his own applications has thus also limited his interpretation to consideration of firstly the placing of maxima and minima along the abscissa, and secondly the <u>relative</u> size of peaks. The present writer concluded that by conforming with this practice he could be confident of remaining within the scope of inference permitted by the technique. Accordingly, where supplementary statistical and graphical procedures have been used below in the course of the description and interpretation of graphs derived by the Thom method, these have been selected to conform to this usage and refer to the placing and <u>relative</u> size of maxima and minima, and not to their magnitude in absolute terms.

It is not claimed that this approach to the analysis was the only one possible. It seems likely to the writer that more sophisticated approaches will be applied in this field in the future, perhaps along some of the lines discussed by Cox and Lewis (1966) or foreshadowed in a brief paper by Bernard (1955). Up to the present, however, the literature of Holocene sea level change has not been characterised by the use of such techniques. As noted above, discussion is generally in non-quantitative terms and graphs have usually been drawn by eye, without consistent allowance necessarily being made for even the published uncertainties of the radiocarbon dates. It was therefore considered that the application of the form of statistical display used here to radiocarbon dates bearing on coastal change represented a worthwhile initial contribution towards more consistent handling of such data.

vi) Procedure in the construction of the main graphs

The principles governing the construction of these graphs were described in v). As was noted there, it was clear that the initial stage in deriving the key curve for each class of data would be marked by considerable visual complexity, since it was necessary to plot several hundred Gaussian curves of varying spreads along the time axis of the graph, each positioned accurately according to its mean value. The scale at which the work was done was therefore considerably bigger than that at which the derived curves are reproduced here. The abscissa was in fact scaled at 100 mm per millennium. The mean values of dates could thus be located conveniently to the nearest five years, and the 10,300 years of the Holocene produced a graph just over a metre long. For convenience of cross reference, this scale was also used for a separate set of graphs, on which the recorded altitudes and strata-thicknesses of the dates were plotted against time, under a regional and site classification. Even at this scale, to eliminate any possibility of confusion or inaccuracy, it was considered best to subdivide the data, and the initial plotting of the 900 Gaussians was spread between some fifteen 1200 mm sheets. A slightly larger number was needed for the altitude and thickness plots.

As noted in iii) above, the standard deviations of most dates are quoted to the nearest 10 years for cases in which the value is less than 200 years. Over 200 years, considerably larger increments are ordinarily used, because their effect on the height of the ordinates is much less (Fig. 5.4). Gaussian curves were therefore prepared at ten year increments for all values from 40 to 200 years, and then for the 250, 300, 350 and 400 year values of standard deviation. In the few cases where quoted values fell between the values covered by these twenty-one curves, they were plotted as the next larger value.

The equation of the normal distribution curve is:

$$y = \frac{1}{\sigma\sqrt{2\pi}} \cdot e^{-\frac{(x-\mu)^2}{2\sigma^2}}$$ (Yeomans 1968)

where σ is the standard deviation

μ is the mean value of the determination and π and e have their usual values as constants (i.e. 3.14159 and 2.71828 respectively).

This equation is such that while the curve gets progressively closer to the x-axis, it never actually touches it. Thus, theoretically, it is never possible to enclose 100% of the area representing probability of occurrence under the curve, no matter how many standard deviations are measured on either side of the mean. In practical terms, however, less than 1% of the probabilities of occurrence lie beyond the range contained by 2.6 standard deviations on either side of the mean, and for practical purposes the distribution is effectively enclosed by the 3 standard deviation limits. Calculations were therefore not carried beyond this point.

Co-ordinates were calculated for the required family of twenty-one equal area Gaussian curves, for stations at the mean value and in ½ standard deviation units from plus 3 to minus 3 standard deviations. Each curve was thus fixed at 13 points. To enhance the accuracy of graphical work, an ordinate scale of double the abscissa scale was used uniformly. The results were checked against Table III in Fisher (1965), and a check was made to ensure that the areas under the twenty-one curves were in fact all equal, despite their variation in spread (e.g. the length of the curves along the abscissa between 3 standard deviation limits varied from 24 to 240 mm, Fig. 5.8)

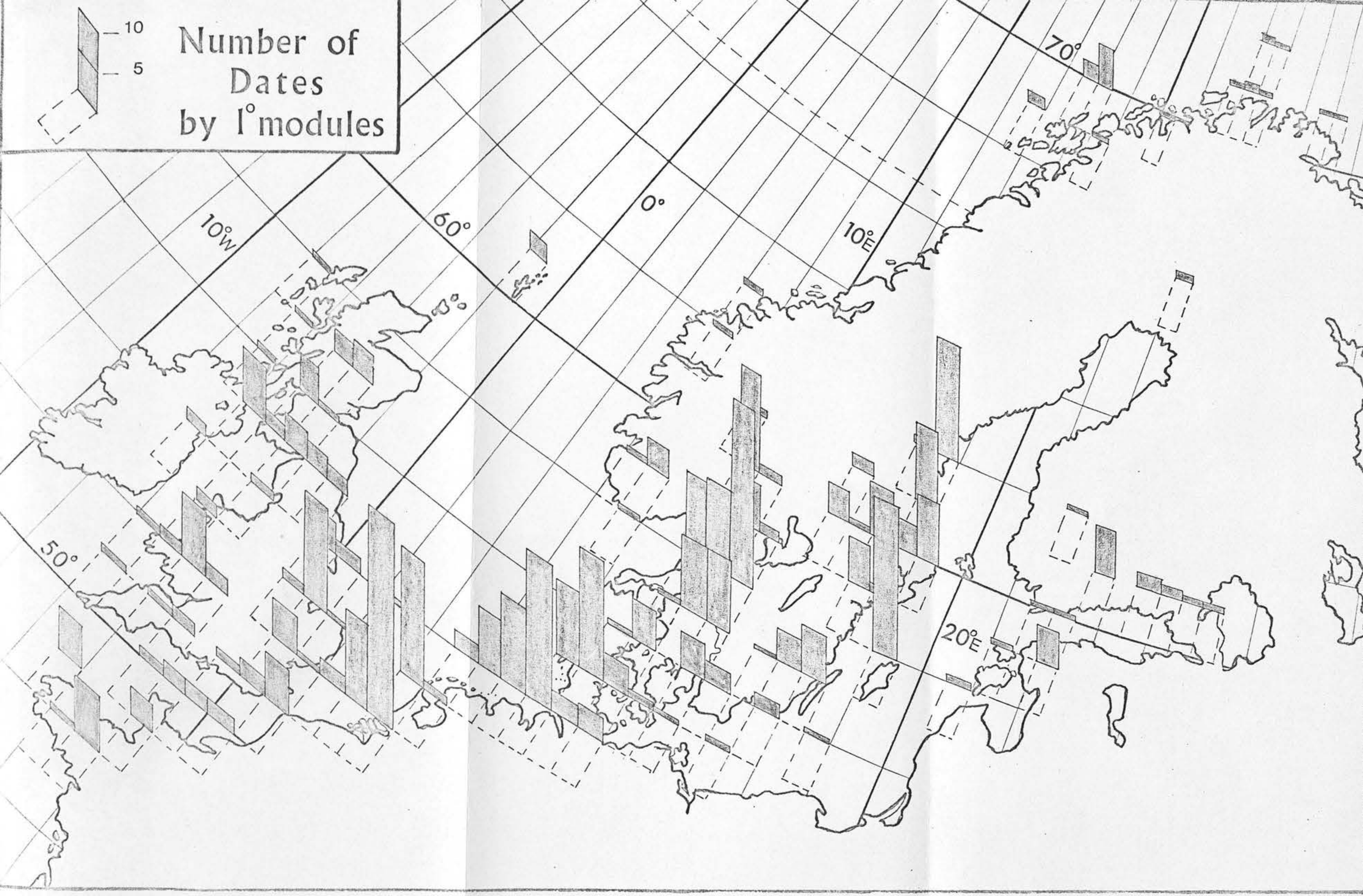
The originals of the family of curves were plotted on a key sheet, working to a quarter of a millimetre. The main graphs were then constructed on tracing paper, using this key sheet as an underlay so that accuracy and speed could be combined in plotting. Checks suggested that plotting errors seldom rose above half a millimetre. They did not appear to occur in a systematic direction (i.e. lines consistently too high, or too low) so that it is unlikely that they accumulated to any serious extent when the ordinates of the curves were summed. Spot checks were made from time to time throughout the course of the work, and the tendency was rather for such errors to cancel out.

The ordinates of all curves in each class were summed at intervals of not more than 1% of the total length of the graph, i.e. at maximum intervals of 1 cm along the abscissa at the scale of the work sheets. Where the direction of the derived graph was changing rapidly, ½ cm intervals were often used. Checks showed that the derived curves could be considered accurate to within the plottable error of the line width used in the diagrams reproduced here.

vii) Summary and conclusion

Despite recent progress in elucidating the relationship between radiocarbon determinations and true age, it remains preferable to regard such determinations strictly as a system of relative dating in the context of the present study. It was ensured that all determinations used here in fact referred to comparable standards. When this standardisation had been carried out in terms of current international practice, the reliability of the data in terms of relative dating appeared adequate for the purposes of the proposed study. Several contrasting types of criteria support this. These ranged from the succession at individual sites to the overall pattern of the evidence through time. The information was classified in geological terms, first in terms of general reliability in indicating coastal change, and then in terms of material and stratigraphy. This classification was restricted to objective criteria, and classifications used by previous workers (e.g. Litorina I, Tapes IV, etc.) were avoided. To help ensure that the data were handled in a

Figure 5.1 Geographical distribution of Western European C^{14} dates relevant to marine changes, by 1° modules.



10
5
Number of
Dates
by 1° modules
70°
60°
0°
10°W
10°E
50°
20°E

Figure 5.2 Chronological distribution of the C^{14} dates and their Standard Deviations.

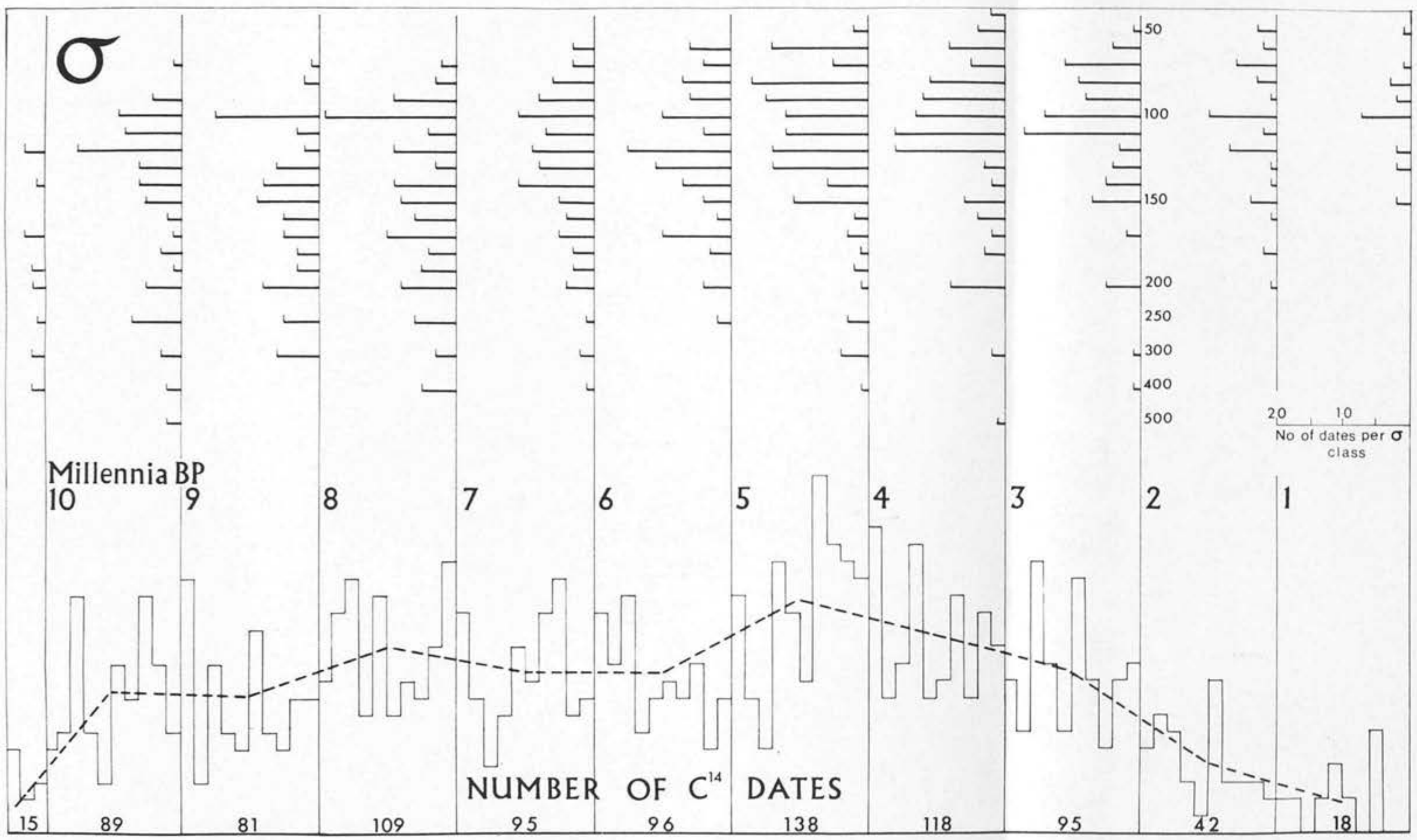

σ
50
100
150
200
250
300
400
500
20 10
No of dates per σ class
Millennia BP
10
9
8
7
6
5
4
3
2
1
NUMBER OF C14 DATES
15
89
81
109
95
96
138
118
95
42
18

Figure 5.3 Total number of C^{14} dates in characteristic Standard Deviation classes.

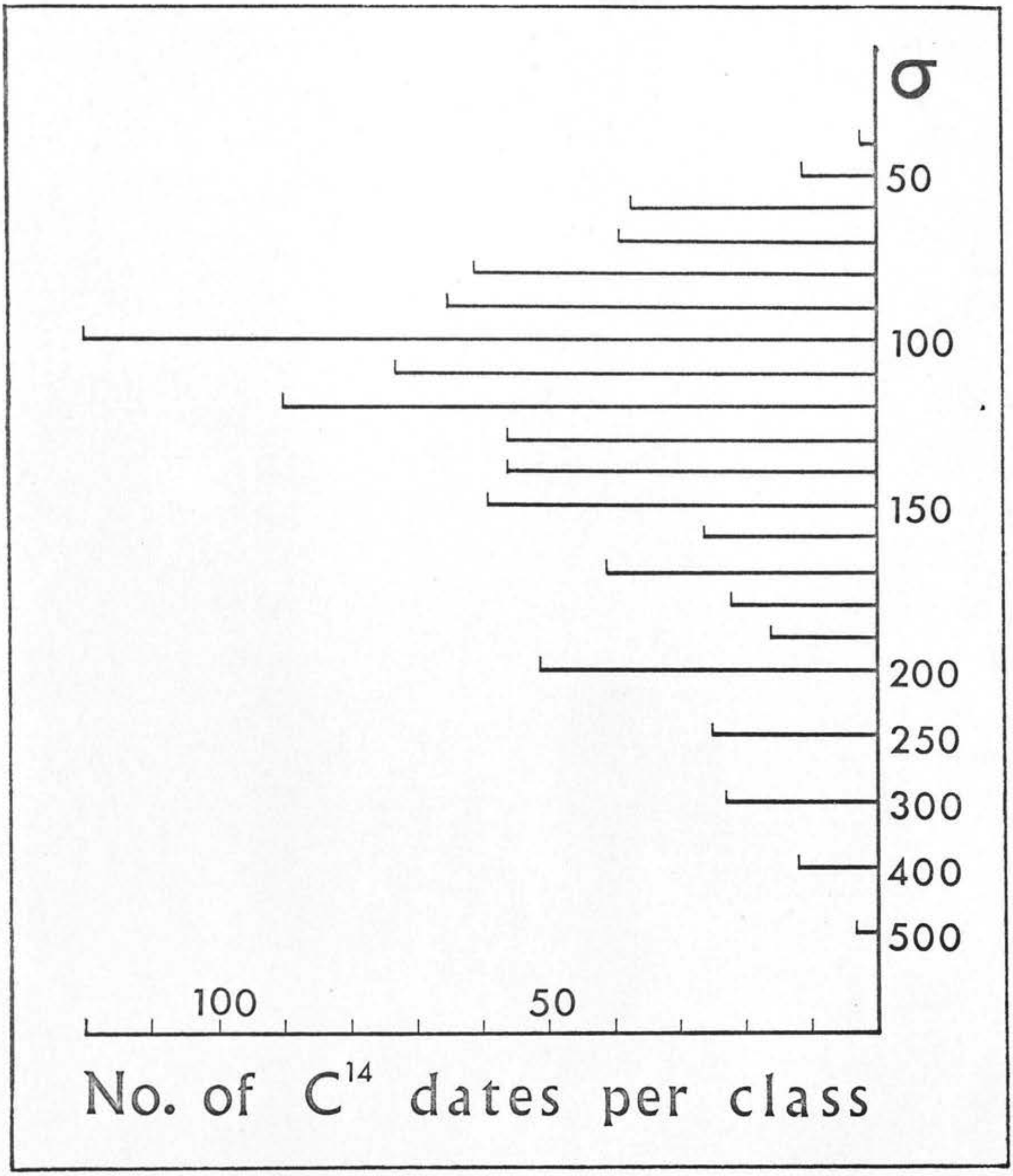
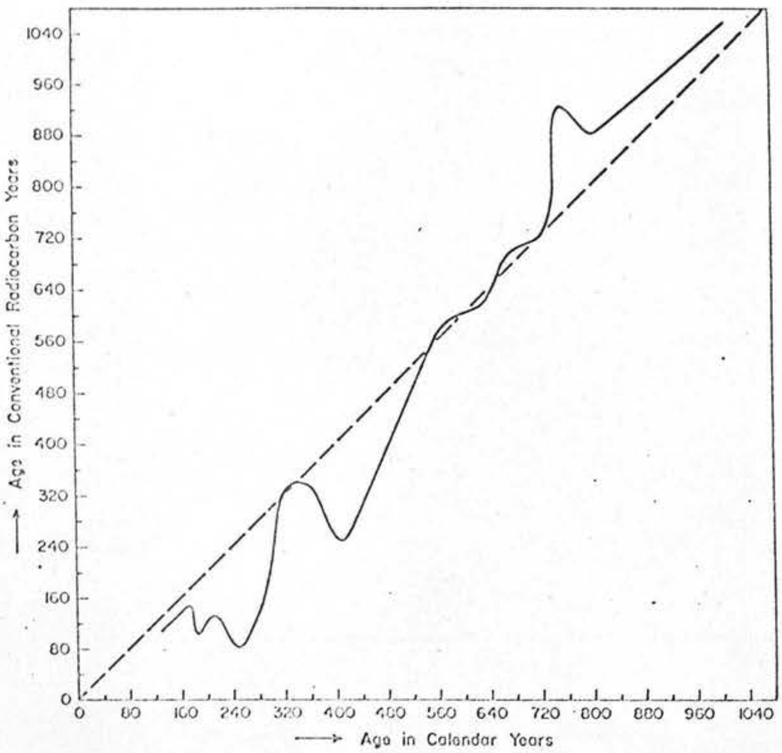

σ
50
100
150
200
250
300
400
500
100
50
No. of C14 dates per class

Figure 5.4 Variations between radiocarbon age and true sample age - after Stuiver and Suess 1966, Fig.1.



1040
960
880
800
720
640
560
480
400
320
240
160
80
0
Age in Conventional Radiocarbon Years
0
80
160
240
320
400
480
560
640
720
800
880
960
1040
Age in Calendar Years

Fig. 5.5 abc Theoretical possibilities in the chronological distribution of C^{14} determinations related to marine transgressions and regressions.

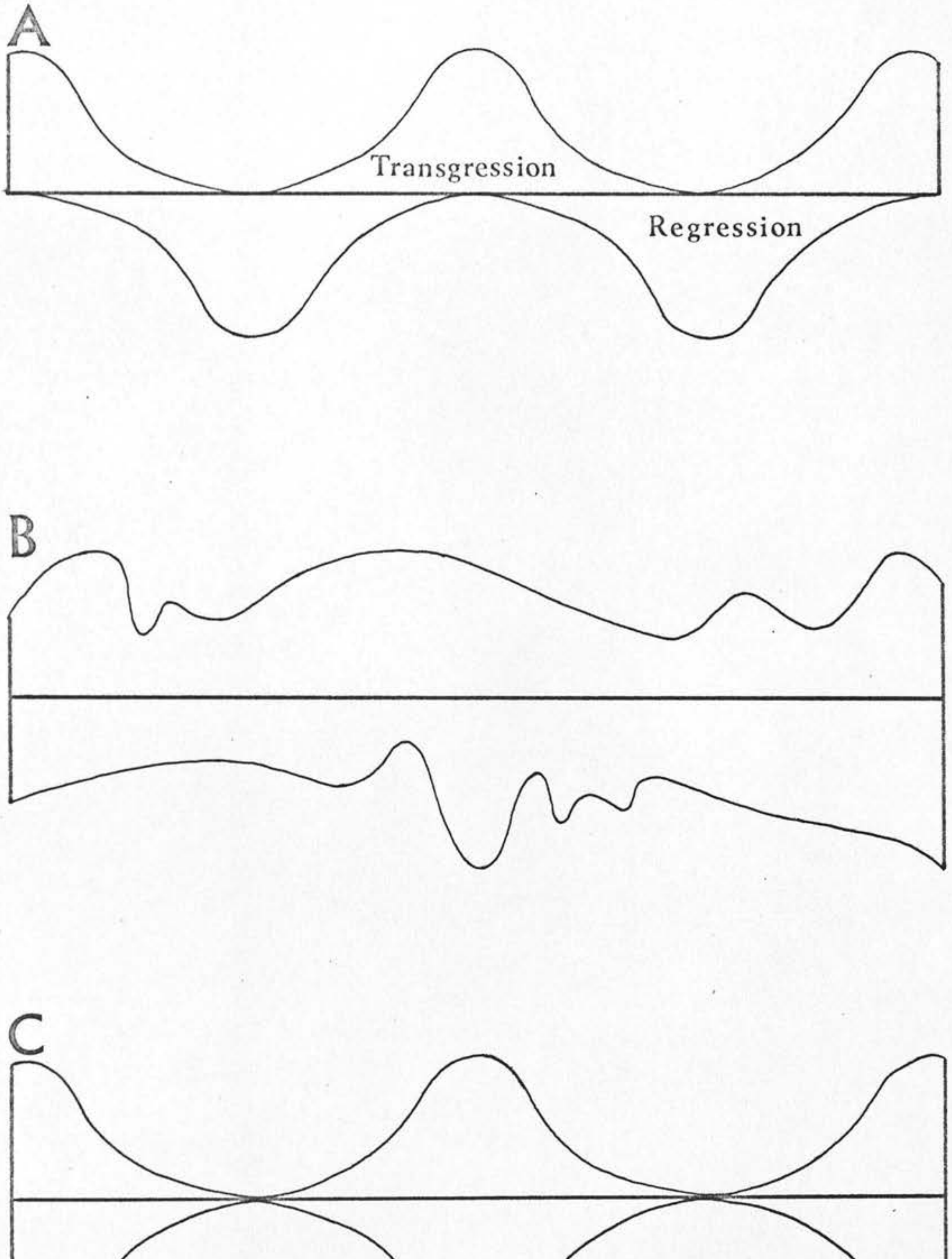
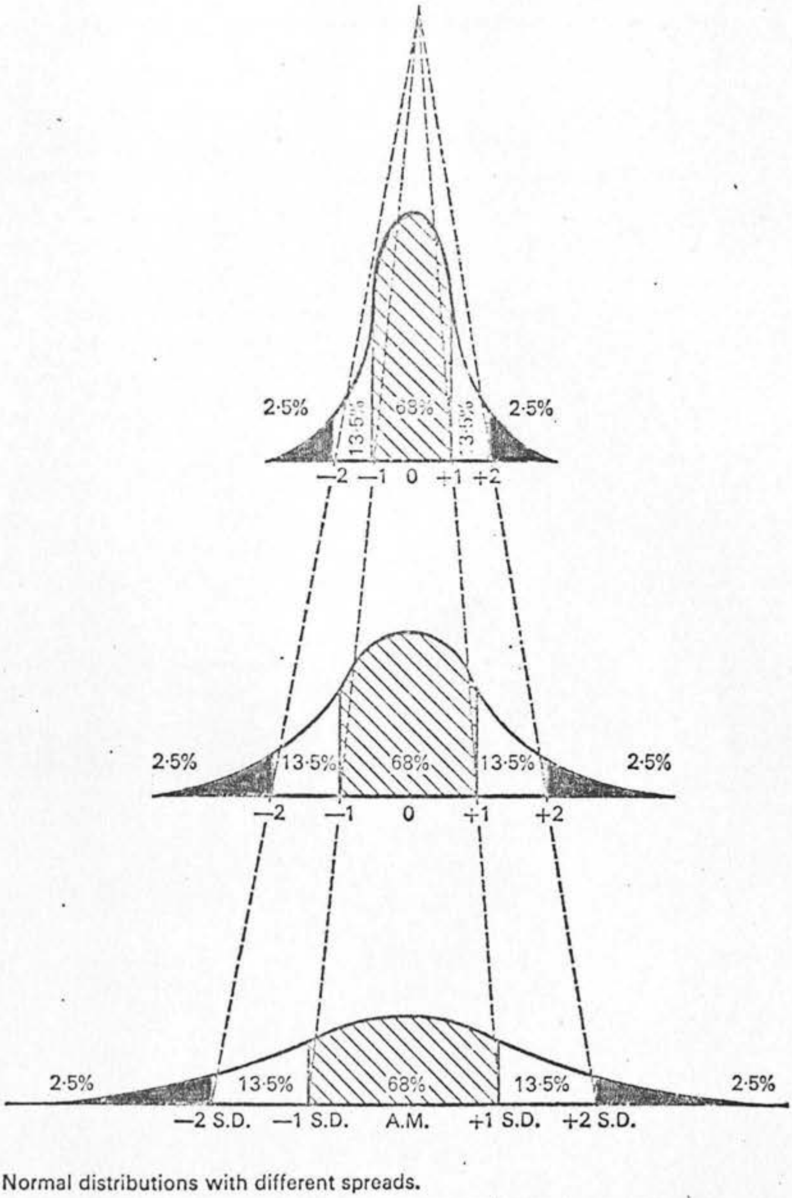

A
Transgression
Regression
B
C

Figure 5.6 Variations in the form of the Normal Distribution curve for different Standard Deviations - after Yeomans 1968, v.II, Fig.2, p.33.



Normal distributions with different spreads.

Figure 5.7 The visual complexity of superimposed Normal Distribution Curves referring to C^{14} dates for marine changes.

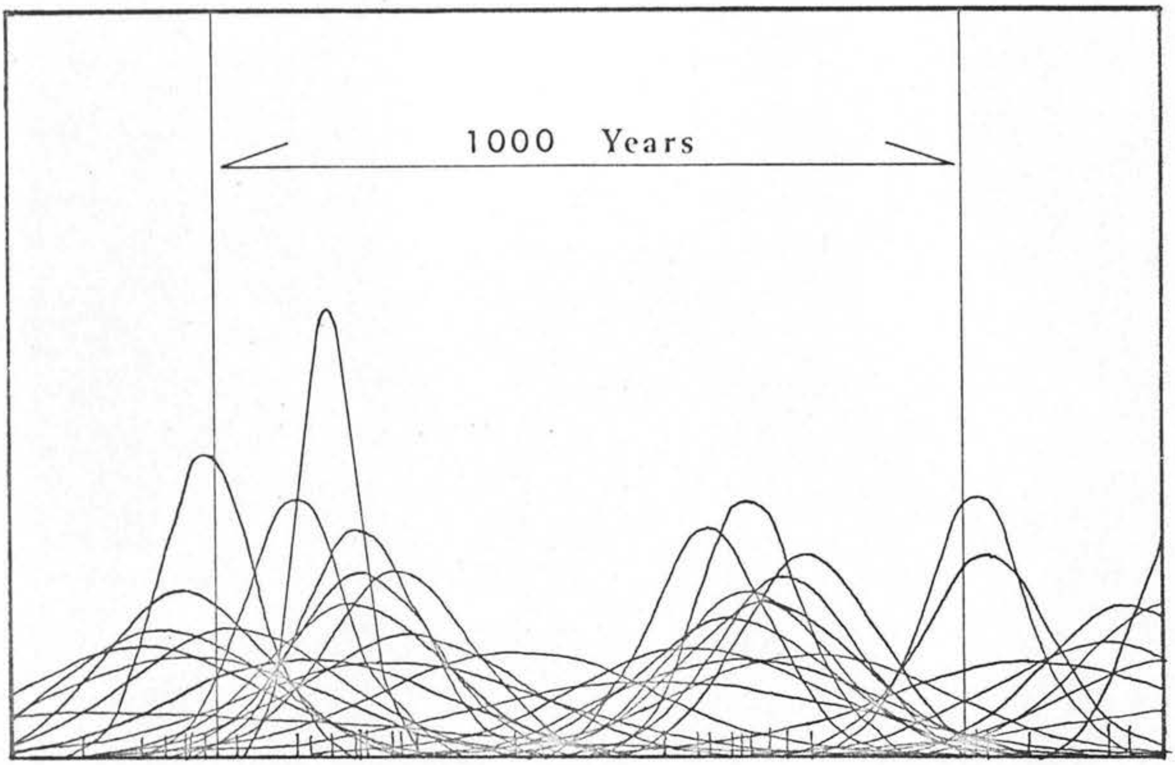
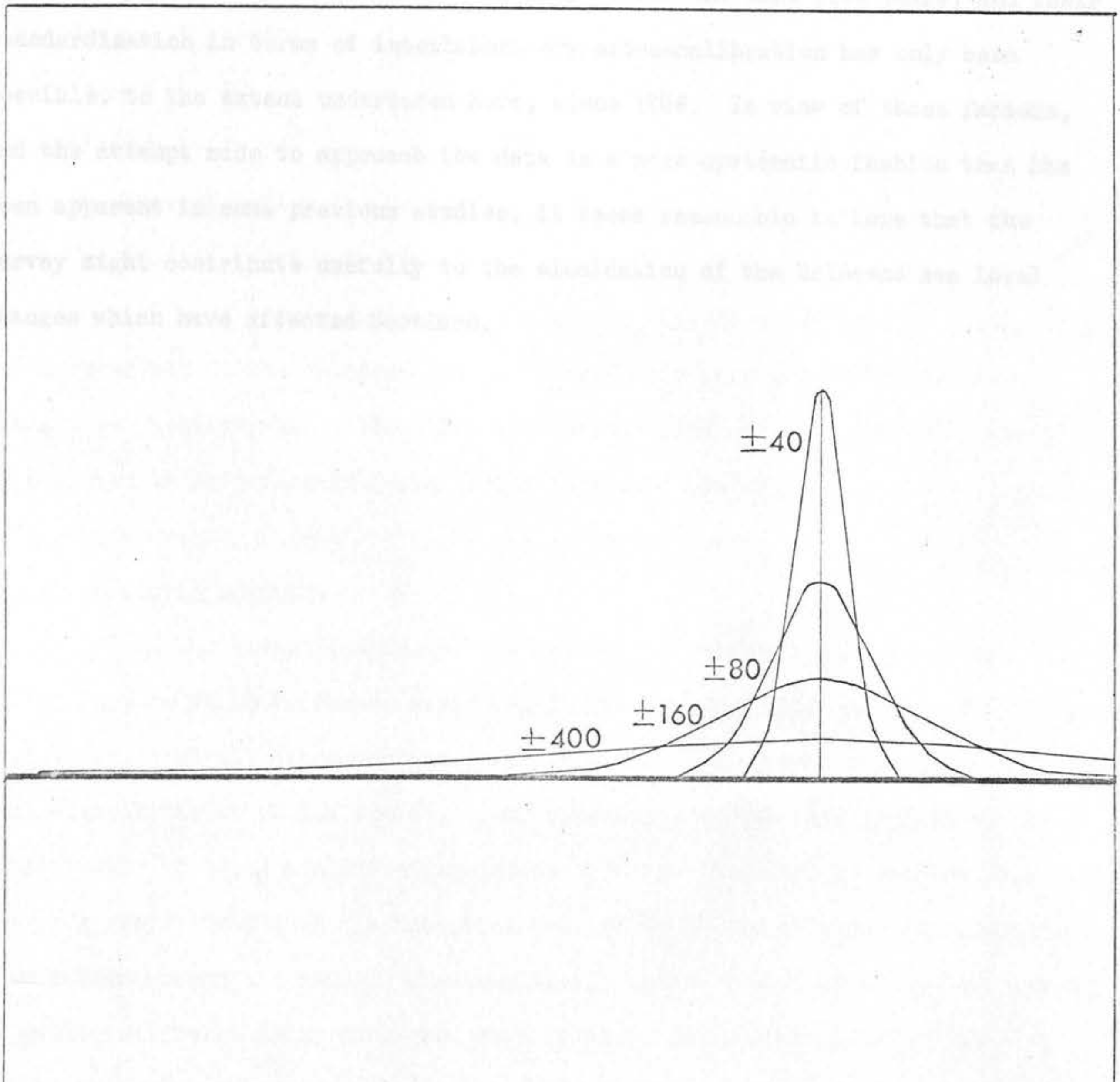

1000 Years

Figure 5.8 Samples of Normal Distribution curves for C^{14} dates with different Standard Deviations (noted in years).


±40
±80
±160
±400

consistent way, a new application of a form of statistical display was devised.

Until very recent years, a survey of the nature undertaken here could not have been attempted. Most of the large number of dates assembled for the present work have become available only within the last five years, and their standardisation in terms of interlaboratory cross-calibration has only been possible, to the extent undertaken here, since 1968. In view of these factors, and the attempt made to approach the data in a more systematic fashion than has been apparent in some previous studies, it seems reasonable to hope that the survey might contribute usefully to the elucidation of the Holocene sea level changes which have affected Scotland.

Chapter 6

The derivation of an initial hypothesis

This chapter contains the following six sections:

i) The approach adopted.

ii) Classes T and R.

iii) Class I.

iv) Class W.

v) The overall pattern of Classes T, W, R and I.

vi) The initial hypothesis.

From this point onwards, unless otherwise specified, all ages and dates refer not to the calendar but to relative age measured in "radiocarbon years", on the Libby half-life. The convention of using A.D. 1950 as "present" is followed in referring to years "before present" (B.P.)

- - - - - - - - - - - - - - - - - -

i) The approach adopted

The classification of the evidence in terms of the geological reliability as an indicator of sea level change was described in Chapter 5 (iv). Altogether, some 435 dates are available from the leading four categories in that classification (T,R,W and I). This represents almost exactly half of the total number of dates available from western Europe (Scotland is omitted from both figures). That such a substantial proportion of the evidence,representing such a high number of dates in absolute terms, could be classed at the top level of geological reliability suggested that it would be feasible to avoid placing weight on lower categories of evidence while framing the initial hypothesis regarding the pattern of Holocene change.

Possible objections to a selective approach of this nature were considered. The most serious was that a spurious result might be produced if subjective impressions arising during the derivation of the pattern influenced the selection of evidence. As noted in the previous chapter, the work was therefore organised in a way designed to avoid this. As indicated in Chapter 5 (iv), the work of classification was completed in its entirety before the construction of the graphs was started, so that the classification allotted to a date reflected only its immediate stratigraphic context and the fullness with which this had been investigated by the fieldworkers. As emphasised above, once the plotting of the graphs was in hand, no retrospective reclassifications were made, and all dates originally included in the classes were plotted without exception. It is thus considered that the selection procedure was independent of considerations arising from time patterns later revealed by the data.

This being so, there appeared to be a strong case for working initially from the classes of evidence most clearly diagnostic of marine change. As will be shown below, several different types of factors have to be taken into account in interpreting the graphs, and it was clearly desirable to exclude further variables where possible. In view of the large number of dates available at the level of Classes T, R, W and I, it was considered that the addition of the geological uncertainties surrounding the lower classes of evidence would tend to reduce the reliability of the initial interpretation more than the addition of their numbers might strengthen it.

The primary objective in interpreting the graphs is to form an initial estimate of how far coastal changes during the Holocene of western Europe reflected eustatic as opposed to local or regional factors. The attainment of this objective is complicated by three main sets of factors. In discussing these it is convenient to use an analogy drawn from communications theory. This is the concept of a division between "signal" and "noise". In the present context, the "signal" which it is desired to receive concerns the nature of the interplay between eustatic and local factors, and the "noise" consists of the effect on the graphs of those extraneous factors which tend to interfere with the interpretation of the "signal". Two of the complicating factors may be described as sources of "noise", and the third relates to the nature of the "signal" itself.

The errors and uncertainties involved in the measurement of the age of the samples represent one source of "noise" on the graphs. The several limitations characteristic of the C^{14} dating technique have been discussed in some detail in Chapter 5 and errors due to unsuspected contamination of samples in the field have also been mentioned there. These also may result in apparent ages which may either be too young or too old. Contamination may for instance be by intrusive younger roots or by the presence of ancient carbon derived from much older deposits and deficient in C^{14}.

Even in the ideal and unlikely circumstance that none of these types of chronometric error is present, a certain level of noise must be expected due to inconsistencies in the nature and location of the samples dated. These are the second set of factors complicating the interpretation of the graphs. For instance, it was noted in Chapter 5 (iv) that while Classes T and R, say, objectively reflect the stratigraphic positions of the samples dated, (e.g. peat at the contact with, respectively, overlying or underlying marine clay), it can not always be guaranteed that the growth of that peat was in fact intimately related to the timing of the local marine change. Either ecological factors or erosion may have caused an unsuspected hiatus in timing.

Even if there were no errors in either the measurement of the date, or the attribution of the sample as an immediate indicator of marine change at the particular point dated, the inconsistent geomorphological placing of the locations from which the samples were drawn would inevitably produce a measure of confusion on the graphs. Ideally, it would be possible to define the timing of transgressions and regressions in terms of their maxima, i.e. by using dates from samples placed geomorphologically to reflect their maximum extent in each locality dated.

Unfortunately, only about 5% of the T and R class dates are located in such terms. The specific sites of the great majority reflect instead the exigencies of field sampling, i.e. the availability of material suitable for dating and the haphazard occurrence of access to such material (often through excavations and boreholes made for other purposes, either academic or commercial.

The possibility of relating date variations and height variations of samples in detail was explored as an approach to reducing "noise", but it was found that too many variables were involved, even in areas like the eastern North Sea coast, characterised by extensive areas with closely comparable stratigraphy. This was so even in restricted time spans, and within areas small enough to allow the presumption that all sites considered had shared closely similar crustal movements. Variations in contemporary environmental factors such as exposure and local differences in tidal range combined with subsequent differences, for instance, in compaction, to give a scatter on time/altitude graphs that seemed too great to allow immediately useful conclusions. Certainly the degree of scatter suggested that this approach did not offer a sound basis for removing "noise" from the graphs.

The third type of factor complicating the interpretation of the graphs involves not extraneous "noise", but the nature of the "signal" itself. As indicated in chapter 4, it seems likely that the interplay between eustatic and local factors was itself complex in Holocene Europe, and that the balance of this interaction varied through time. Although in the graphs a rapid major eustatic rise or fall could be expected to dominate the effect of coastal changes due to local factors, it is a reasonable presumption that some changes due primarily to non-eustatic causes were in progress throughout the period, and that even at phases dominated by eustatic events some dates included in the graphs would refer to events which departed from the general eustatically controlled trend because of special local conditions.

Not only during periods of eustatic still stand but at phases when changes in ocean level were gradual or small in amplitude, the proportion of changes due to independent local or regional factors might be sufficiently large to dominate the graphs. The problem of identifying and disposing of the "noise" component is thus increased by the fact that in the signal itself the division between phases with and without eustatic control can not be assumed to be clearcut.

In view of this it seemed advisable to obtain as comprehensive and as well defined a view as possible of the time patterns. It was therefore concluded that it was preferable to consider the pattern represented by Classes T and R separately, before adding Classes W and I to the graphs and considering the top level of evidence as a whole. Classes W and I certainly also represent local increases and decreases, respectively, in marine influence, and W could thus be taken to correspond broadly to T, and I to correspond broadly to R. However, it seemed best to avoid preconceptions regarding the exact nature of these relationships, and to examine first the relationship between the two classes with the clearest implications, T and R.

<u>ii) Classes T and R</u>

It will be recalled that these classes contain samples appearing to date transgression (T) and regression (R) contacts. Together, Classes T and R make up just over two thirds (68%) of the total of all dates classed at the highest level of geological reliability, the 176 dates of Class T represent

41% of that total and the 119 dates of Class R, 27%. These dates, and all others used in the calculations leading to the graphs of the present chapter, are identified individually in Chapters 8, 9 and 10, while the sources and corrections of their values are set out in the Appendix.

Figures 6.1 and 6.2 show the curves representing the overall pattern of their evidence through time, derived according to the procedure set out in Chapter 5, (v) and (vi).

In each of these figures, it will be noticed that prior to 6000 B.P., the evidence from the Baltic is shown separately. This is because during parts of that period the Baltic was a separate waterbody, independent of the sea. This will be discussed below. It will suffice here to note that the dating of the periods at which this was so is in dispute, but that there seems general agreement that the Baltic has been connected to the oceans ever since at least 7000 B.P. 6000 B.P. has therefore been used as a safe dividing line in all graphs in this chapter. A more detailed division between lake and sea phases is considered in the next chapter. Thus, although the Baltic evidence prior to 6000 B.P. is shown here for completeness, its discussion will be deferred until then. Unless the contrary is specifically stated, the following paragraphs therefore refer to the evidence from the western seaboard of Europe outside the Baltic prior to 6000 B.P., but include the Baltic evidence after that date.

The curve summing the Class T dates, Fig. 6.1, extends continuously from 10,000 B.P. to within 300 years of the present. Its most obvious characteristic is its sharp undulations. Before 4000 B.P., the heights of the major peaks are often between six and nine times the heights of the intervening minima, and even after that date the modulations remain marked. The highest of the Class T peaks (at ca. 3700 B.P.) is more than twice the height of the succeeding minimum, and over twenty-five times the heigh of the preceding one. The radiocarbon dates thus exhibit a well-marked series of groupings, rather than an even distribution through time.

The curve summing the Class R dates, Fig. 6.2, is less complete in its coverage. It starts at 10,300 B.P. and finishes at the present day, but except in the Baltic, no evidence is available between 8700 and 7250 B.P. The curve is again markedly undulatory in character, although with one exception the differences between heights of maxima and minima are less extreme prior to 4000 B.P. than in the case of the Class T evidence. This exception is however the dominant feature of the Class R curve. It is a considerable grouping of regression contact dates centred about 4300 B.P. This peak is over two and a half times the height of the highest of the other Class R maxima (ca.5400 B.P.) and some one and three-quarter times the height of the largest Class T maximum. The size of this concentration of Class R dates will be discussed in detail later, but it may be noted here that it is somewhat inflated by non-geological factors.

Between 2000 B.P. and 500 B.P., the ratio of the heights of maxima and minima is more extreme in the case of the Class R than the Class T curve. The number of dates contributing to the R curve is however unusually small in this sector, and the sharpness of the undulations probably reflects only the discontinuity of the data. It is the tendency towards grouping exhibited in periods when larger numbers of dates are available that is the most interesting feature of both the T and R curves.

As indicated in Chapter 5, many factors might account for groupings of this nature, and many of the possibilities (notably side effects of C^{14} fluctuations) would be difficult to eliminate by considering the curves individually. As suggested in Chapter 5 (iii), however, the relationship of the two sets of patterns is informative.

If the grouping of the dates had been dominated by effects arising from internal characteristics of the radiocarbon phenomenon, it could be assumed that both T and R dates would show essentially the same grouping since both classes consist of closely similar biological material (in many cases T and R samples included in the present survey were indeed cut from opposite sides of the same peat bed, within a few centimetres of each other. That this possibility may essentially be eliminated is demonstrated by Fig. 6.3. In this the graphs of the T and R classes of evidence are superimposed, and it is evident that the maxima of the two classes do not tend to coincide. On the contrary, maxima in each class would appear to tend to fall at the same times as minima in the other.

From Fig. 6.3, then, it would seem that these two curves have a broadly reciprocal relationship. This can be shown more clearly in visual terms if the curve of Class R is in fact inverted relative to that of Class T (Fig. 6.4). It will be seen that 48 of the turning points on the graphs may be paired reciprocally (i.e. maxima with minima) without any discrepancy within a pair being as large as 250 years. Indeed in 19 out of the 24 pairs the difference in age between corresponding turning points is a century or less.

Of the 14 maxima and minima that can not be paired (marked X on the graphs) it will be noticed that the majority are closely associated with the sectors of the abscissa on which only one and not both of the classes of evidence is represented, i.e. only the three inflexions marked Y appear to present clear contradictions between the patterns of the Class T and Class R evidence. At these points, either the T curve or the R curve is close to the abscissa and in fact these discrepancies depend essentially on only one date, in each case. As will be shown below, the dates involved should not be dismissed out of hand. Nevertheless, the agreement between the patterns of the Class T and Class R evidence seems notably complete.

If for present purposes, conflict is defined as the situation where (with the R curve inverted) the gradient of the curves disagrees in sign (that is, periods when the amount of R evidence increases instead of decreasing as the amount of T evidence increases) and vice versa, then conflict between the curves is only apparent over some 30% of the ten millennia under study. Many of the sectors of the pattern showing conflict are of short duration. More than a third are less than 50 years, and more than half less than 100 years long. Only in 6 sectors are conflicts apparent over periods of more than 150 years, and these cases together occupy some 17.3% of the period under study. Thus, taking into account the additional 17% in which there is no opposing evidence, there is no conflict in the graphs of the T and R classes over 70% of the period under study, and no disagreement lasting longer than 150 years in 83% of the period.

As indicated in Chapter 5 (iii), the writer found the clear nature of this pattern both surprising and encouraging. A less well-defined pattern had been anticipated in view of the range of potential sources of "noise", and the possibility that independent timing of local events might outweigh the eustatic component in the "signal". The nature of the pattern that in fact emerged provided the basis for a more clearly defined initial hypothesis than had been expected when the survey was first undertaken.

It has already been shown (Fig.5.5 cf. Fig. 6.4) that the two classes of dates are not grouped in a way that would suggest that their pattern was altogether dominated by vagaries of the C^{14} process. If the two curves had exhibited no clear relationship, but had been characterised by unrelated fluctuations, it would have been difficult to ascertain how far this was due to the influence of eustatic fluctuations being outweighed by local factors in coastal change, and how far it reflected the various sources of "noise" combining to reduce the fidelity of the dating below the level of resolution required by the prevailing rate of sea level change. The extent to which increases in evidence of transgression in fact correspond to decreases in evidence of regression, and vice versa, throughout the greater part of Holocene, suggests however that these factors did not dominate the pattern of the graphs, either individually or in combination. It would seem a reasonable preliminary conclusion that the pattern reported here is likely to reflect eustatic change. Before discussing this further, the evidence from Classes W and I will be introduced.

iii) Class I

Classes W and I together contain some 32% of the total of dates classed at the top level of geological reliability.

The 89 dates of Class I represent almost two thirds of the number from these two classes (21% of the overall total). When the sum graph for Class I evidence was plotted, like the graphs for Classes T and R it was found to be characterised by marked undulations (Fig. 6.5). The results of the comparison between the T and R curves (Fig. 6.4) suggested that gross C^{14} "noise" effects could probably also be ruled out as the dominant cause of the grouping of the Class I dates, and geological causes were therefore considered.

Although the geographical distribution of T and R dates extends over the whole western European seaboard, the Class I dates all refer to Fennoscandia. Both the Class R and Class I dates imply a local reduction of marine influence at the points. The possibility exists that land uplift was a determining factor in the case of some R dates. However, the fact that a large proportion of Class R dates are drawn from sites where relative rise of sea level has dominated the Holocene sequence suggests that the graph of that class is unlikely to be dominated by factors arising from land uplift. This possibility seemed much stronger in the case of the Class I dates, because of their restriction to areas where isostatic uplift has dominated the Holocene sequence.

The grouping of these dates through time might hence reflect irregularities in isostatic uplift. This possibility has long been considered in Scandinavia. The various possible types of irregularity have formed the subject of much discussion, since they were proposed by such workers as (inter alia) von Post (1929; 1947) and Florin (1944). However, E. Nilsson's conclusion of 1958 that there is no generally accepted proof of irregularities of any kind in the isostatic land uplift would appear to be accepted in most recent literature. This is certainly so in Sweden, the source of most of the Class I dates. As Lundqvist (1965) summarised the situation, majority opinion regarding Scandinavian crustal movements is that these were rather smooth and regular. He considered (op.cit. p. 174) that "the complications are probably all of eustatic nature", but added "however, opinions about these very complicated problems will certainly change more than once". It would thus appear that though the possibility that the graph of Class I evidence is dominated by variations in uplift can be regarded as improbable, it can not be entirely eliminated in terms of the Scandinavian literature.

The graph of Class I evidence (Fig. 6.5) is discontinuous prior to 6000 B.P., when the Baltic evidence was admitted, but it is then continuous to within 400 years of the present day. The graph of Class R evidence is also shown in this figure, and it is evident that the two patterns differ in many respects. There are nevertheless many similarities. The number of peaks is approximately the same, and although several disagree, there is a broad correspondence in timing if not in relative size (it is however notable that the biggest Class I peak, ca. $2\frac{1}{2}$ times the height of the next largest, coincides with the peak that dominates the Class R graph). It would seem unwise to attribute too much significance to the appearance of a double peak at A in Fig. 6.5, since this is made up of only two dates (st 1475 and St 776), whose ranges overlap at $1\frac{1}{4}$ standard deviations. If this grouping of Class I evidence at A is taken to correspond to the peak at B in the R evidence, only the peak C appears to represent a definite conflict between the two curves. The other three peaks (marked D) which do not match, fall at periods for which only one of the classes of evidence is available.

The remaining 20 peaks in the graphs may be grouped in pairs as shown. No pair contains a discrepancy of more than 250 years. Seven out of the ten pairs agree to within 150 years, and three of these to within less than 50 years. In light of this, it is not surprising that the main effect of adding the Class I evidence to the Class R graph is to accentuate the existing peaks in that graph (Fig. 6.5, upper curve). No Class R peaks are cancelled out, and only one new peak is added. This is centred on 8650 B.P., i.e. in a period for which no Class R evidence is available.

The starting point of this comparison was the fact that both classes of evidence represented decreases of marine influence at the sites dated. In view of the agreement in the timing of the two categories and the widespread distribution of the Class R evidence across different regimes of land movement, it seems unlikely that the grouping of Class I dates reflects episodes of irregular land uplift. The relationship between the categories suggests that the dominant factor in the timing is indeed more likely to be eustatic change.

The possibility that the dates representing the isolation of lakes from the sea were systematically earlier or later than the general class of regression contacts (i.e. Class R) was investigated. It will be recalled that the closest interval ordinarily used in plotting the graphs was ½% of the length of the abscissa, i.e. ca. 50 years. In the 10 pairs of peaks on the graph, on three occasions the I curve peak occurred within 30 years or less of the R curve peak. It was accordingly considered best to disregard these cases. In the four of the remaining seven cases where the R peak fell before the I peak the total discrepancy amounted to 510 years, as opposed to 530 years in the cases where the opposite situation prevailed. It is considered that no geologically meaningful trend may be discerned in this.

The relationship between the curve representing the sum of the I and R evidence and the Class T curve was then examined. It was found that the IR curve provided a slightly better match for the Class T curve than the R curve alone. The differences were however small (in no case did the addition of the Class I dates displace a Class R peak by more than 100 years), and they will be considered together with those due to Class W when the pattern of T R I and W evidence is considered as a whole, below.

iv) Class W

The 50 dates of Class W represent 11% of the total number of dates classed at the top level of geological reliability. This class is thus the smallest at this level, being only about half the size of Class I and little more than a quarter of Class T. It is also much the most restricted in geographical distribution, the samples being drawn almost entirely from the Netherlands, where the interest of this class of material for studies of sea level change was first established.

It will be recalled that the material is a basal Holocene peat, lying unconformably on Pleistocene deposits and overlain by marine clay.

Comprehensive work by Saskia Jelgersma (1961) suggested that the accumulation of this peat indicated a watertable rising under marine influence. The aim here is therefore to assess the relationship of this evidence to the Class T dates. The basal peat itself is essentially made up of freshwater vegetation. This is certainly so of the samples included in Class W. These came from the bottom of the peat, where it rested on the Pleistocene material.

Other samples were often taken at the same sites from the contact between the top of the peat and the overlying marine clay, and these samples are included here in Class T. A comparison of pairs of W and T dates from the same sites showed that the period between the start of accumulation of the basal peat and its final submergence by the sea was generally several hundreds of years and sometimes more than two millennia. The curves of T, R and I evidence all suggested relatively rapid fluctuations of marine influence in the Holocene. The length of the W/T timelags at individual sites suggested that the peat often continued to accumulate through more than one cycle of marine influence. This in turn suggested that the relationship between the time patterns of the W and T evidence might well be obscure, and that the onset of accumulation of the basal peat might reflect essentially local factors. On the other hand, as Jelgersma has shown (op.cit., and Fig. 6.6), these dates form a notably coherent pattern on a time-depth diagram. This would be difficult to account for if they did not in fact closely reflect marine change. Various possibilities were therefore examined.

The distribution of the Class W dates through time is graphed in Fig. 6.7 (shaded). It extends almost continuously from ca. 8350 B.P. to ca. 2400 B.P. On first inspecting the pattern in relation to the T-curve (Fig. 6.7 also), the way that peaks in the Class W evidence tended to avoid the maxima of the Class T evidence was noted. The possibility was therefore considered that Jelgersma's time-depth observations were correct, i.e. the dates did reflect marine change, but that the Dutch theory of the relationship might be wrong. The relative patterns of the T and W curves seemed to suggest that the accumulation of the basal peat might be initiated in regressive phases. Comparison with the R-curve (Fig. 6.7) showed only one pair of coincident peaks however (ca. 3500 B.P.), and this involved only one out of the 50 Class W dates. On the contrary, the Dutch interpretation appeared to be confirmed by the comparison, at least in the 2800 year stretch of graph from the start of the main part of the Class R curve at 7250 B.P. towards its main peak in the four thousands. In this stretch, which contains some four-fifths of the Class W dates, the peaks of the R-curve tend to correspond to troughs in the W-curve.

The possibility that the time distribution of the W evidence might indeed be related to the pattern of Class T was therefore re-examined. It was noticed that much of the W distribution could be accounted for if it was considered that the accumulation of basal peat tended to start neither during the peaks of transgression evidence nor during the peaks of regression evidence, but towards the beginning and end of the transgressive peaks.

Thus, the groupings of Class W dates about 8000 and 7250 B.P. correspond to the beginning and end of a major Class T peak; the W maxima at 6250 and 5750 B.P. to the ends of the next two major Class T peaks; the W maxima at 5050 and 4650 B.P. and ca. 3900 and 3500 B.P. to the beginnings and ends of the next two again; and that at 2800 to the beginning of another. This interpretaion would in fact account for all but two of the 50 Class W dates.

Of these, only that at ca. 6750 B.P. appears to be entirely anomalous on this scheme. That at 4270 B.P. overlaps to within one standard deviation with a transgression contact date that makes a small peak in the T-curve. This could only be taken to conform to the sub-pattern exhibited by the W-peaks at 5050 and 4650 B.P., which each coincide with small peaks superimposed on the general peak of Class T evidence between 5400 and 4500 B.P.

It might be suggested with some plausibility that the relationship between the Class W and Class T dates could be explained on the hypothesis that the watertable peat tended to start accumulating not when rising sea level was affecting the land at its greatest rate but when the rise was proceeding more slowly, i.e. during minor fluctuations and as major transgressive cycles waxed or waned. A search of relevant Dutch literature produced no evidence in conflict with this (e.g. inter alia: Jelgersma op.cit., Florschutz 1954; Pons, Jelgersma, Wiggers, de Jong, 1963; de Jong, 1965; Hageman, 1969), and as will be shown later (Chapter 12), this interpretation of the relationship between Class W and Class T evidence has the merit of offering grounds for reconciling Jelgersma's smooth time/depth curve of sea level change with other recent western European curves which appear to be founded on equally valid observations but which exhibit an oscillatory pattern. Although the interpretation suggested here is thus of interest, it must in the writer's view remain a matter for discussion.

Whether or not it is correct, however, on empirical grounds it would seem that there is in general a positive relationship between Class W and Class T evidence. If the Class W dates are added to the graph of Class T, of the nine Class T peaks affected (Fig. 6.7) eight are increased in height. Out of the eight, only in the case of the peak at 2700 BP is the increase at the maximum outweighted by the increase at one of the adjacent minima (ca. 2900 B.P.) Six on the contrary show a positive increase in amplitude (i.e. the height difference between the maximum and the flanking minima is increased). The decrease in amplitude between 2700 and 2900 B.P. depends on only one Class W date, whereas the largest increase in amplitude (between 5900 and 5400 B.P.) coincides with the greatest abundance of Class W dates.

The two peaks in the combined TW-curve which do not correspond to peaks in the T-curve (ca.6750 and 3500 B.P.) each also depend on only one Class W date. The anomalous appearance of the date at 6750 B.P. within the proposed scheme has already been noted above as has the fact that the date at 3500 B.P. is exceptional in that it is the only Class W date which coincides with a Class R peak. The 3500 B.P. date certainly falls towards the end of a major peak in the Class T curve, and thus conforms in principle to the scheme, but a rapid double alternation of Class T and Class R peaks occurs between 3650 and 3350 B.P., and it thus seems advisable to avoid placing much weight on a peak which depends solely on the precise placing of one date in this narrow range. The effects of including and omitting each of these dates are both shown (dotted) in the graphs (Fig. 6.7 et seq.)

With the exception of these two dates, the addition of the 50 Class W dates tends to accentuate the basic pattern presented by the other 385 dates from the upper level of the data included in the survey. This is true even in the case of the only peak in the Class T distribution cancelled out by the Class W evidence (at 7000 B.P.), for this was one of those which found no counterpart in a minimum in the Class R evidence. As in the case of the addition of the Class I and Class T evidence, the changes in timing caused by the addition of the Class W and Class R evidence are small. The effect of the combination of these changes on the overall pattern of the evidence will next be considered.

v) The overall pattern of Classes T, W, R and I

From the above, it would seem that, even if aspects of interpretation remain matters for discussion, it may be accepted that the categories of evidence representing decreases in marine influence (i.e. Classes R and I) follow related patterns through time and that the same is true of those representing increases (Classes T and W), though the relationship is there more complex.

It has already been shown in (ii) above, that there is a strong reciprocal relationship between Classes T and R. It remains to be seen whether when Classes W and I are added this relationship is blurred by the greater geological diversity of the evidence, or clarified by the increase in the overall number of observations.

The patterns of the curves summing the T and W evidence and the R and I evidence are shown in Fig. 6.8. Some 17% of the period of study was not covered by both curves when only the T and R evidence was admitted, but this is reduced to 12% in the case of the combined TW and RI curves.

The number of turning points which can not be paired reciprocally is also reduced, from 14 to 11 if those introduced solely by the two anomalous Class W dates are disregarded. Of these, eleven (marked X), five are closely associated with sectors of the abscissa where only one class of evidence is represented. The remainder define the three inflexions marked Y, which correspond to the three on the T-curve which were marked Y on Fig. 6.4. In both cases, these represent the only clear contradictions between the patterns of the pairs of graphs.

In the case of the T and R curves, 48 turning points could be paired reciprocally, with a maximum discrepancy of just less than 250 years. In the combined TW and RI graphs, 52 turning points may be paired, and the maximum discrepancy is reduced to 180 years. In 21 out of the 26 pairs (as opposed to

19 out of 24 pairs) the difference in age between turning points is of the order of a century or less.

If "conflict is again defined in terms of the signs of the gradients of the curves, as in section (ii), then the combined TW and RI curves are in conflict for just under 25% of the ten millennia under study (as opposed to 30% in the case of the T and R curves). However only on four occasions (as opposed to six) are conflicts apparent over periods of more than 150 years, and these cases together occupy only 9.6% (as opposed to 17.3%) of the period under study. There was thus no conflict in the graphs of the combined classes over 75% of the period under study and no disagreement lasting longer than 150 years in 90.4% of that period, as compared to figures of 70% and 83% respectively when the Class T and R evidence was considered alone.

It thus seems clear that the addition of the further 139 radiocarbon dates of Classes W and I to the 295 in Classes T and R clarifies the pattern of alternations, rather than blurring it.

Hence it seems likely that the grouping of dates in each of these four classes of evidence tends to be dominated by a common factor. The strict alternation of peaks of evidence indicating, successively, increase then decrease of marine influence at the sites dated can not in the writer's view be accounted for convincingly in terms of any of the sources of "noise" discussed above. Furthermore although the 11% of the dates in Class W are from a restricted geographical area, even the 21% in Class I cover a considerable variety of amounts of land uplift across Fennoscandia, while the 68% of the total in Classes T and R are distributed throughout the whole western European range of positive and negative land movements and contrasting coastal environments. It thus seems unlikely that either local environmental factors or regional land movements could be so uniform in their effect on the heterogeneous sites dated as to produce the pattern which has been demonstrated. On balance, then, it is considered that the common factor dominating the observed pattern of the T W R and I dates is most likely to be eustatic change of sea level.

By stating this it is not meant to indicate that the graphs do not imply either "noise" or purely local or regional changes in marine influence. On the contrary, very few of the ordinate values of minima approach zero and in many cases they represent a substantial proportion of the height of their adjacent maxima. This indicates that "noise" and local effects are indeed present. All that is suggested here is that the strict alternation of successive increases and decreases in marine influence that dominates the graphs is most simply explained at this stage in terms of the synchronising influence of eustatic change. As indicated in (i), after the special case of the Baltic evidence has been considered in Chapter 7, the evidence of all classes of dates will be adduced in Chapters 8, 9 and 10, and the validity of this provisional hypothesis will be examined in detail.

vi) The initial hypothesis

The pattern of the evidence that has been considered in the present chapter is summarised graphically in Fig. 6.9. The upper and lower curves will be recognised as the sums of T and W and R and I classes respectively. The disagreement between the corresponding maxima and minima of these curves have already been considered in detail. Full allowance was made for them in calculating the two curves shown nearer the abscissa.

Each of these may be regarded individually as a line summarising the overall pattern of the dates making up the four categories and classes at the top level of geological reliability in this survey. It will be noticed that their turning points correspond exactly in timing, as do the points where they cut the abscissa. The curve that lies consistently nearest to the abscissa follows a mean course, bisecting the distance between the TW and RI curves at each point in time.

The derivation of the other curve is slightly more complicated, but in some respects it provides a more informative summary of the evidence than the mean. The principle used is of a type described by Burrows (1965). The classes of evidence above and below the abscissa represent, respectively, increases and decreases of marine influence. Ideally, if eustatic change of sea level was the only influence recorded, no regressive evidence would fall in sectors of the abscissa occupied by peaks of transgressive evidence, and vice versa. The former case is in fact illustrated between 8200 and 7250 B.P., but this is not typical. As was indicated in the first paragraph of this section, very few of the ordinate values of minima in fact approach zero. How far this departure from a purely "eustatic" pattern is made up of extraneous "noise" and how far of genuine but local changes of marine influence which are in opposition to the eustatic trend can not be distinguished at this stage. However, additional indication of the relative importance through time of the combined "non-eustatic" elements in the evidence may be gained by subtracting the ordinate values of the TW and RI curves at each point of time, instead of averaging them, as in the case of the mean curve.

This was how the final curve of the graph was derived. Thus, instead of following a course halfway between the TW and RI curves, this "difference" curve lies closest to whichever dominates the pattern of the evidence at any given period. Only at points of time when the ordinates of the TW and RI master curves are of equal magnitude does it coincide with the mean curve. When no competing "noise" or local effects are present (as between 8200 and 7250 B.P.) the difference curve coincides with the dominant curve, but when (as for instance between 2000 and 3000 B.P.) these factors are of importance, the amplitude of the variations in the difference curve is much less than the distance separating the master curves.

For convenience, from this point the two derived curves shown in Fig. 6.9 will be referred to as the Mean and the Difference Curves, distinguished by capitals. The Difference Curve will be used again at later stages, when detailed conclusions are drawn concerning the relative importance of eustatic and non-eustatic factors. For the moment, it will serve to note the variations in clarity of the evidence in general terms from Fig. 6.9.

It will be seen that until just before 6000 B.P., "noise" and divergent local coastal changes appear to have little effect on the curves, which are dominated by evidence of transgressions. This pattern is also broadly followed by the Baltic evidence, omitted here before 6000 B.P. but considered in Chapter 7. From 6000 B.P. until just after 4000 B.P. the influence of the "non-eustatic" factors varies, but is least important at the peaks of regressive evidence which dominate this part of the graph. From then until the present day, phases dominated by transgression and regression have been of approximately equal importance. In this final sector, the amplitude of the Difference Curve's oscillations decreases, both in absolute terms and relative to the importance of "non-eustatic" background.

As was noted in the opening paragraph above, the turning points of the Mean and Difference Curves correspond exactly with each other in timing and represent the best compromise between the dates of the corresponding maxima and minima of the TW and RI curves. These turning points therefore offer a convenient way of defining the successive alternations of marine influence which characterise the evidence considered in this chapter. The table below is constructed on that basis. It should be considered in conjunction with the statements made in the previous paragraph regarding the varying importance of "non-eustatic" factors.

With this qualification, the sequence proposed here, and summarised in Fig. 6.9, represents the writer's initial hypothesis of Holocene sea level change, as represented on the western seaboard of Europe by the 435 radiocarbon dates considered most reliable as indicators of changes in marine influence.

The validity of the sequence, and the provisional presumption that it reflects primarily eustatic control, will be examined in detail in the chapters that follow.

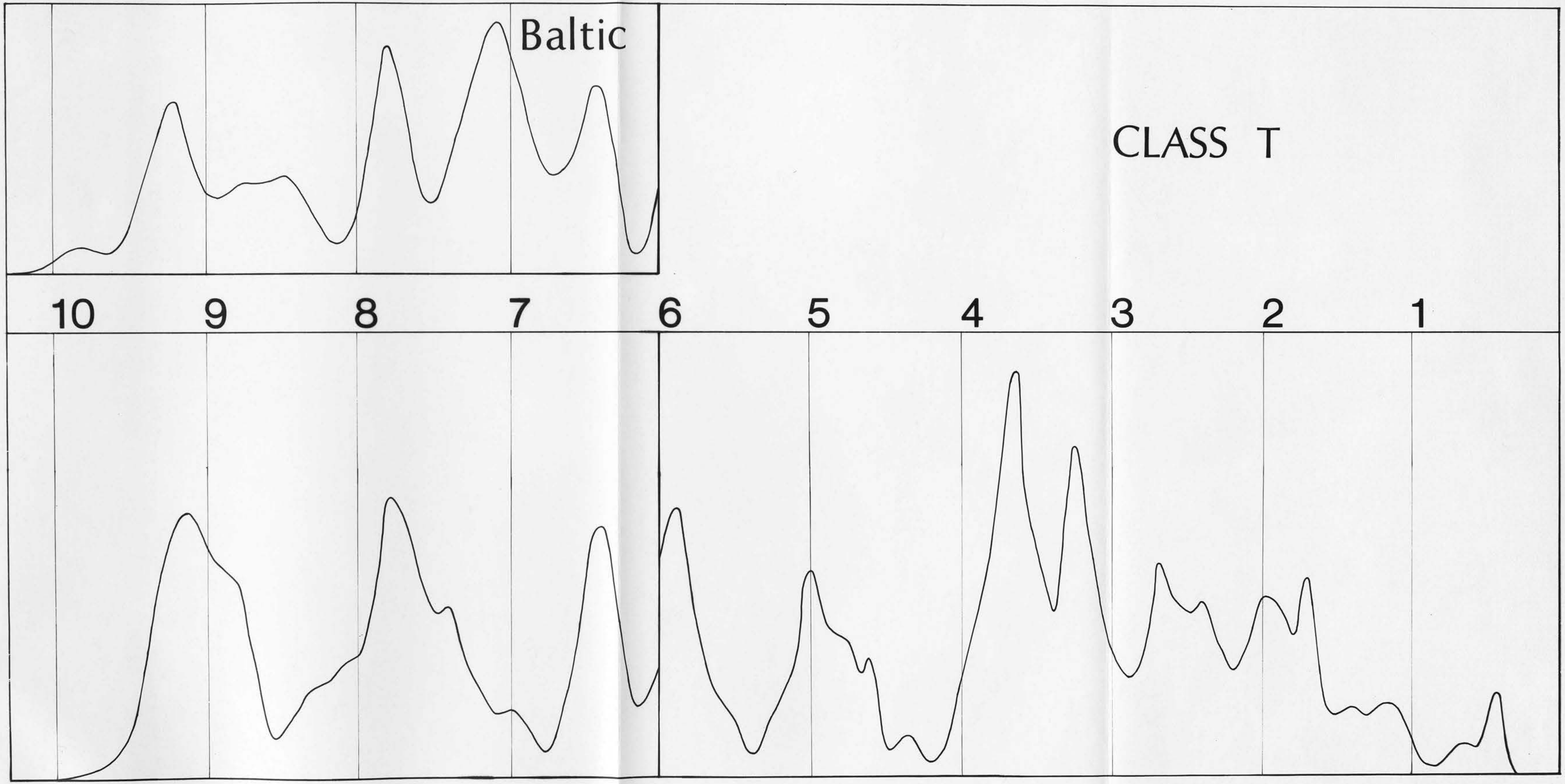
All dates are quoted to the nearest half-century in years B.P., and without exception all turning points of the Mean and Difference Curves are listed.

Phase	Maximum of Transgression Evidence	Maximum of Regression Evidence
A	10000	9750
B	9150	8550
C	7800	7500
D	7400	6800
E	6400	6150
F	5900	5400
G	5000	4900

cont'd.

Phase	Maximum of Transgression Evidence	Maximum of Regression Evidence
H	4750	4300
I	3650	3450
J	3250	2900
K	2700	2500
L	2300	2150
M	1900	1800
N	1700	1400
O	1150	850
P	450	300

Figure 6.1 Chronological distribution of Class T radiocarbon dates (in this and all succeeding graphs, the abscissa represents C^{14} time. Vertical lines divide it into millennia, which are numbered in thousands of years B.P.)



Baltic
CLASS T
10
9
8
7
6
5
4
3
2
1

Figure 6.2 Chronological distribution of Class R radiocarbon dates.

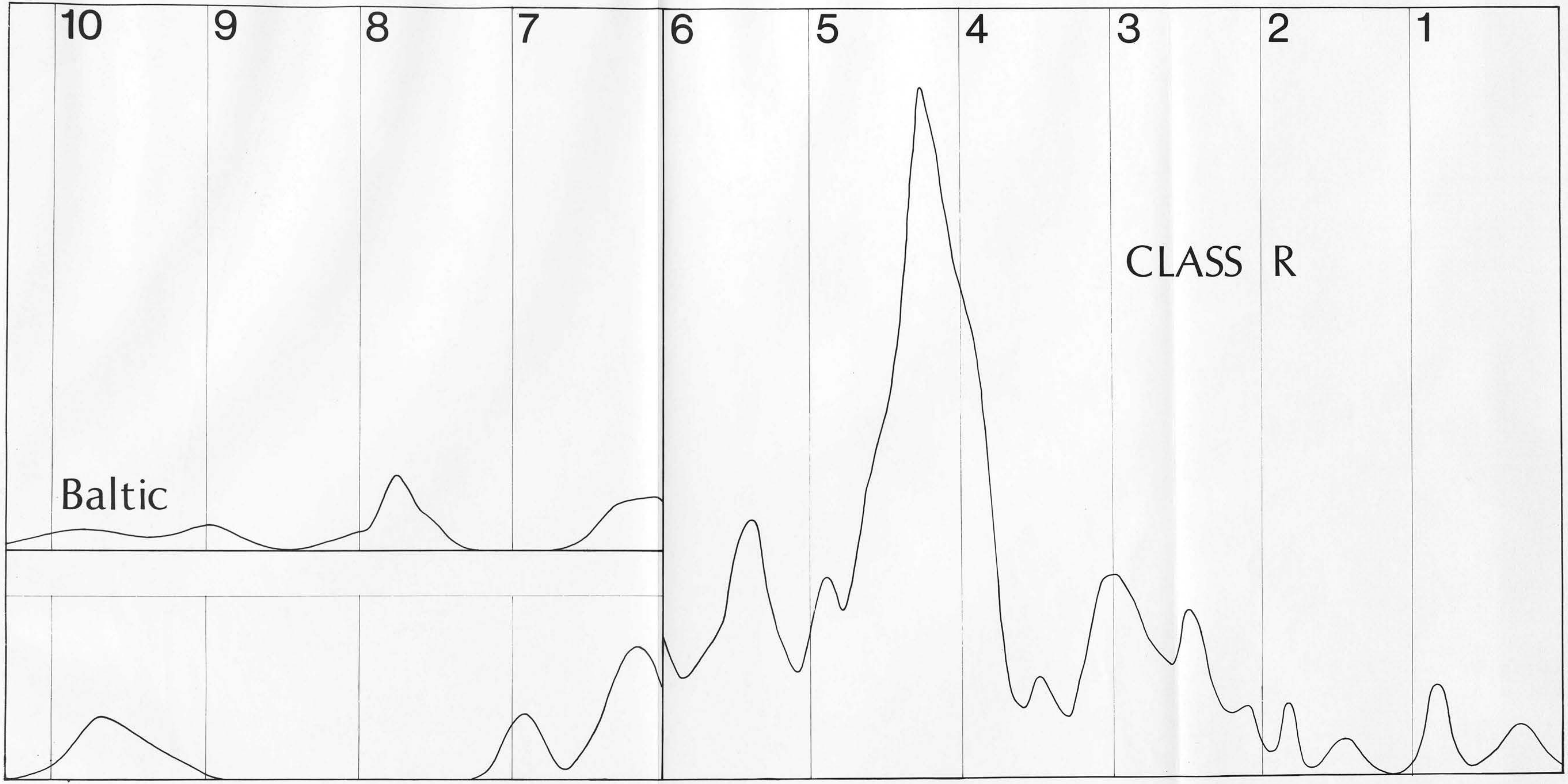

10
9
8
7
6
5
4
3
2
1
CLASS R
Baltic

Figure 6.3 Classes T and R superimposed.

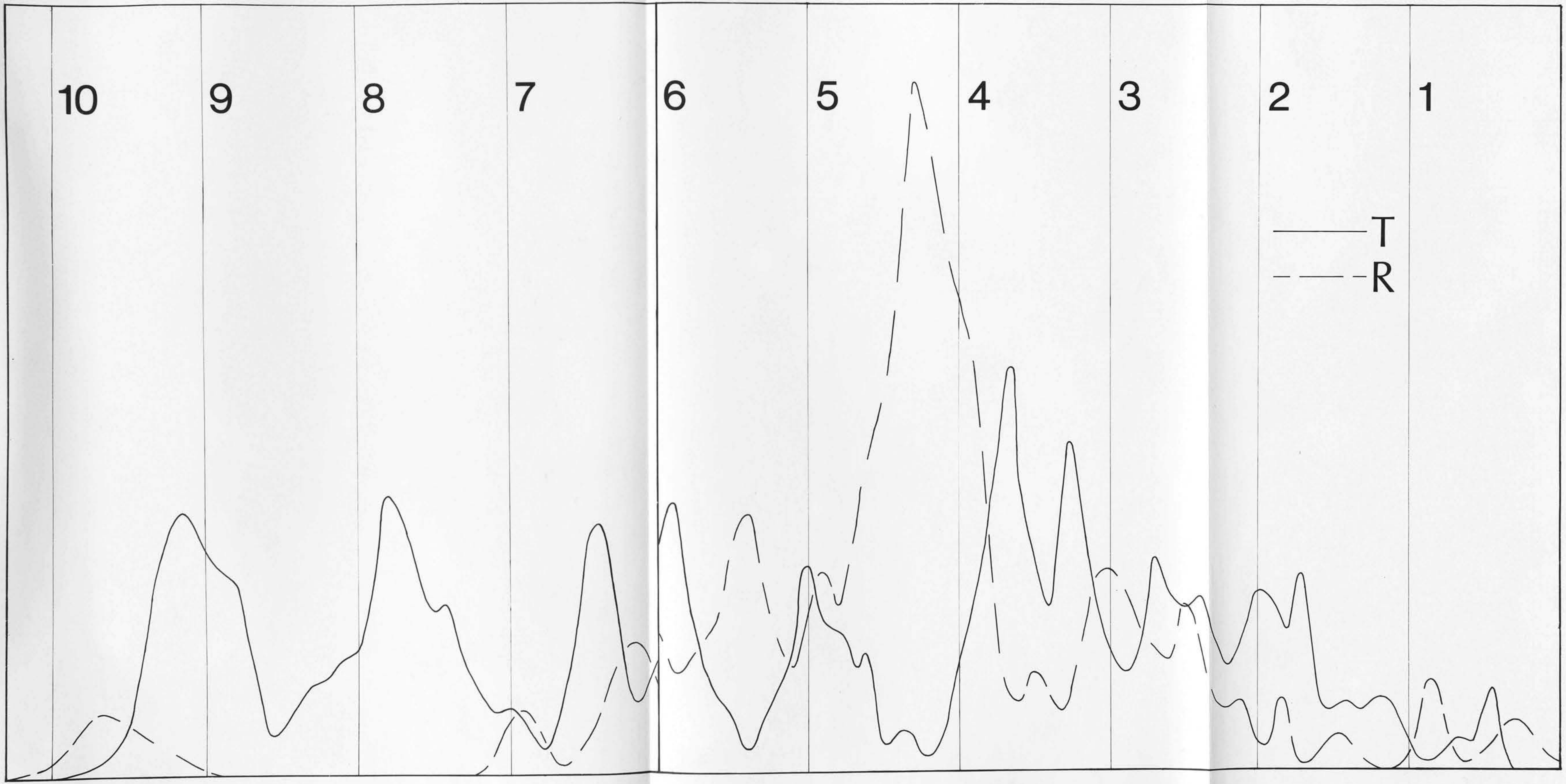

10
9
8
7
6
5
4
3
2
1
T
R

Figure 6.4 The Class R curve inverted **relative** **to** the Class T curve.

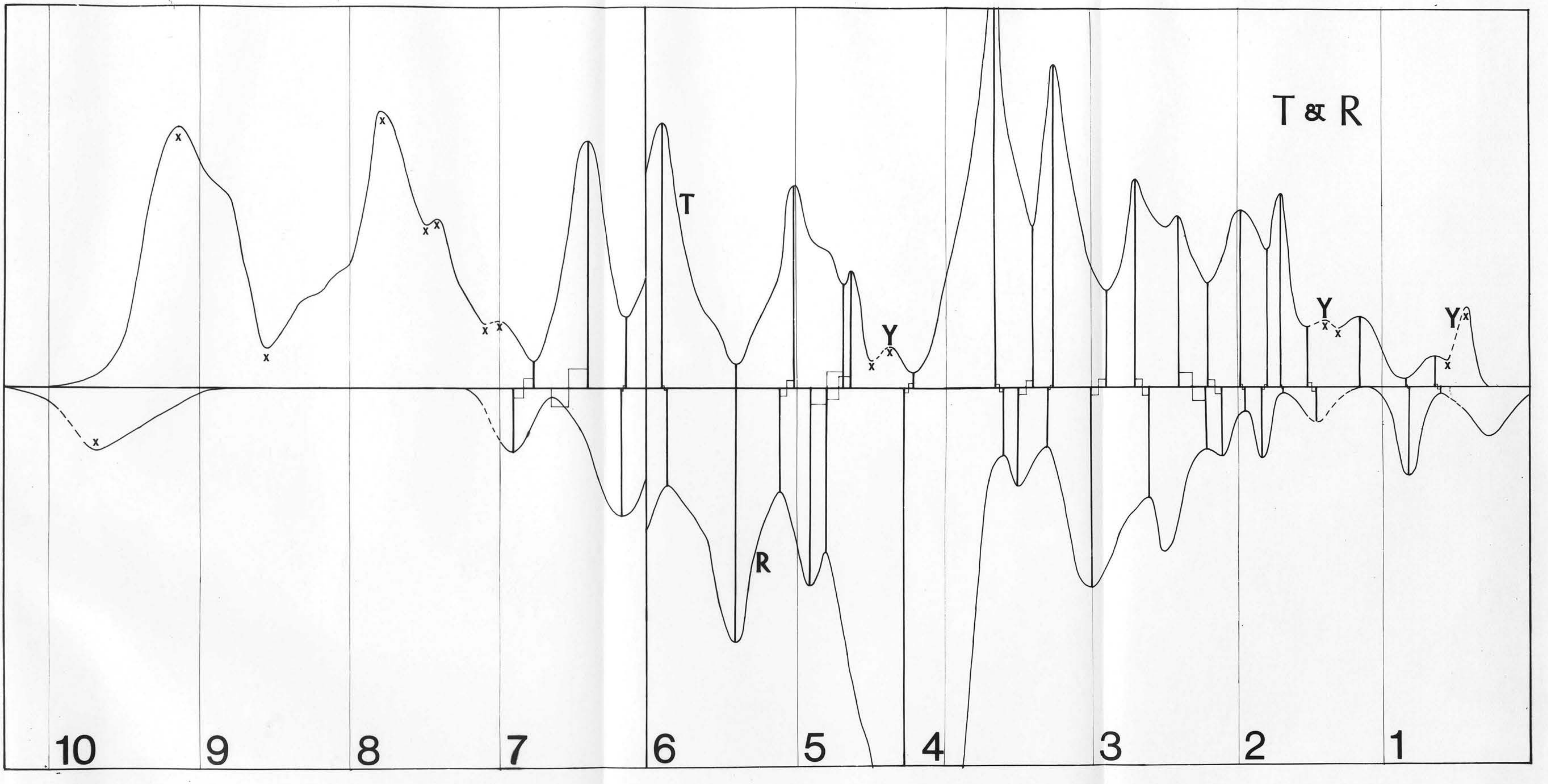

T & R
T
R
Y
Y
Y
10
9
8
7
6
5
4
3
2
1

Figure 6.5 Comparison and summation of Classes I and R.

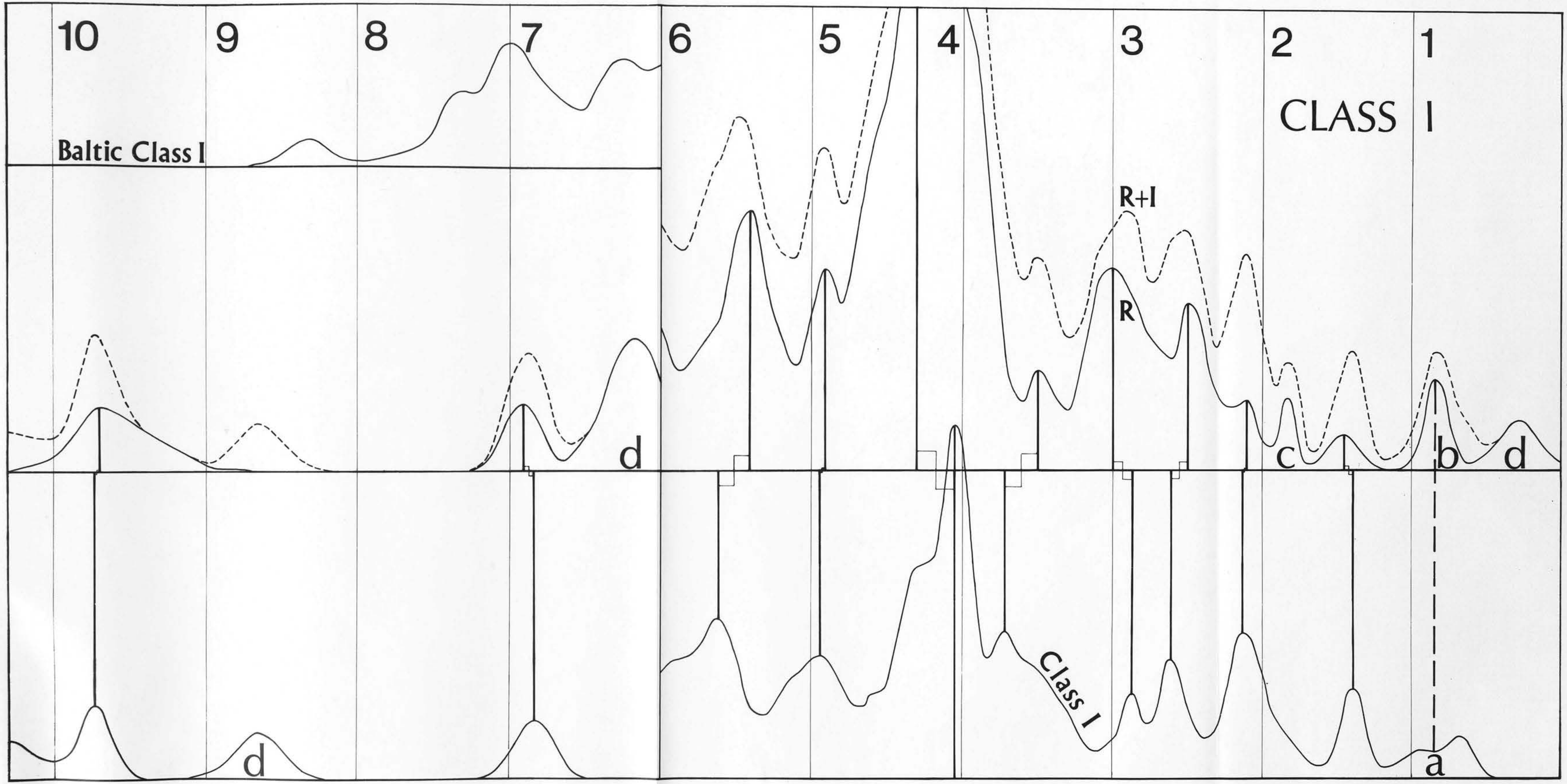
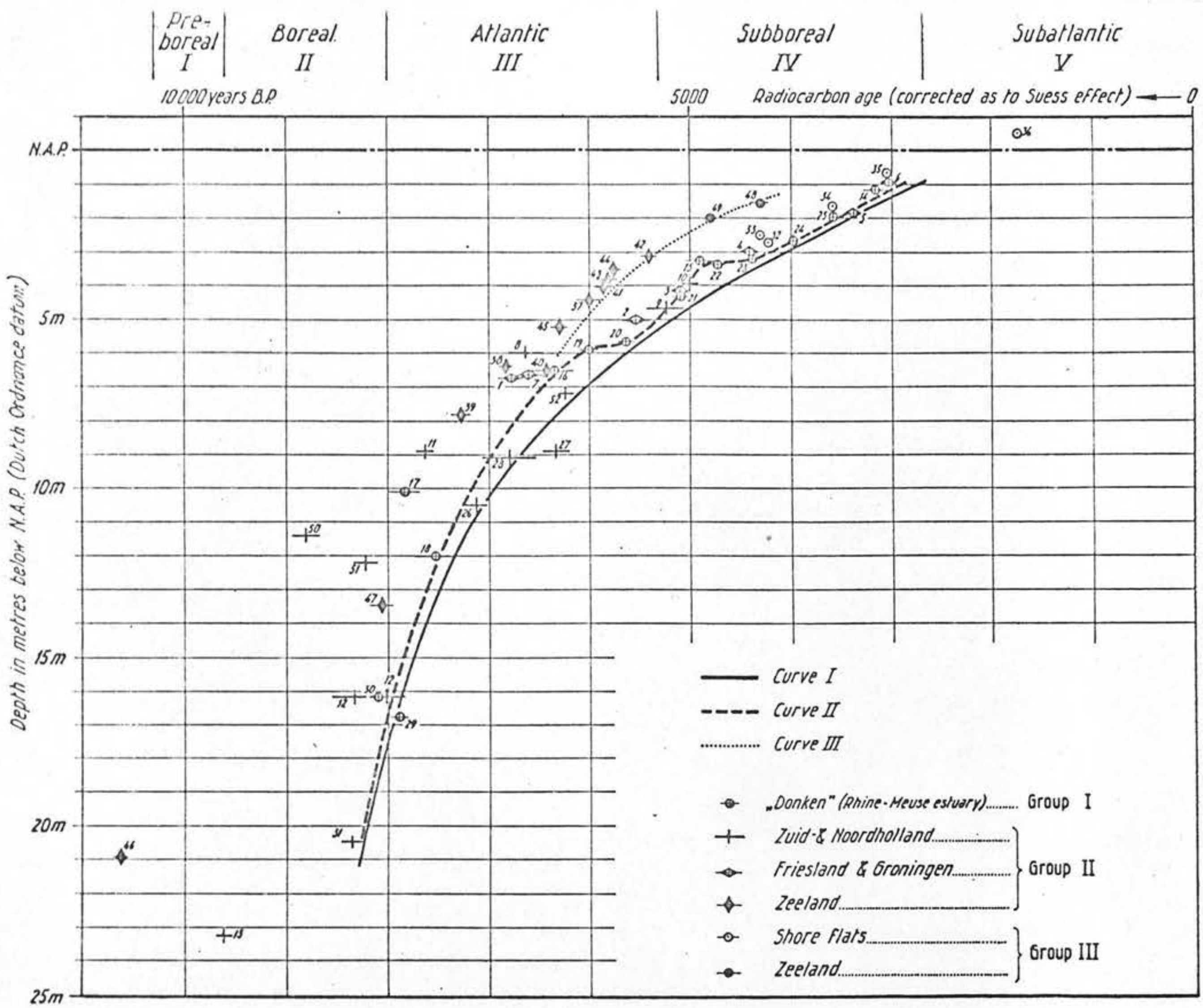

10
9
8
7
6
5
4
3
2
1
CLASS I
Baltic Class I
R+I
R
d
c
b
d
d
Class I
a

Figure 6.6 Dutch data, with sea level change curve proposed by Jelgersma, 1961, Fig.22, p.44.



Pre-boreal I
Boreal II
Atlantic III
Subboreal IV
Subatlantic V
10000 years B.P.
5000
Radiocarbon age (corrected as to Suess effect)
0
N.A.P.
5m
10m
15m
20m
25m
Depth in metres below N.A.P. (Dutch Ordnance datum)
Curve I
Curve II
Curve III
„Donken" (Rhine-Meuse estuary) Group I
Zuid- & Noordholland
Friesland & Groningen
Zeeland
Group II
Shore Flats
Zeeland
Group III

Figure 6.7 Comparison and summation of Classes W and T.

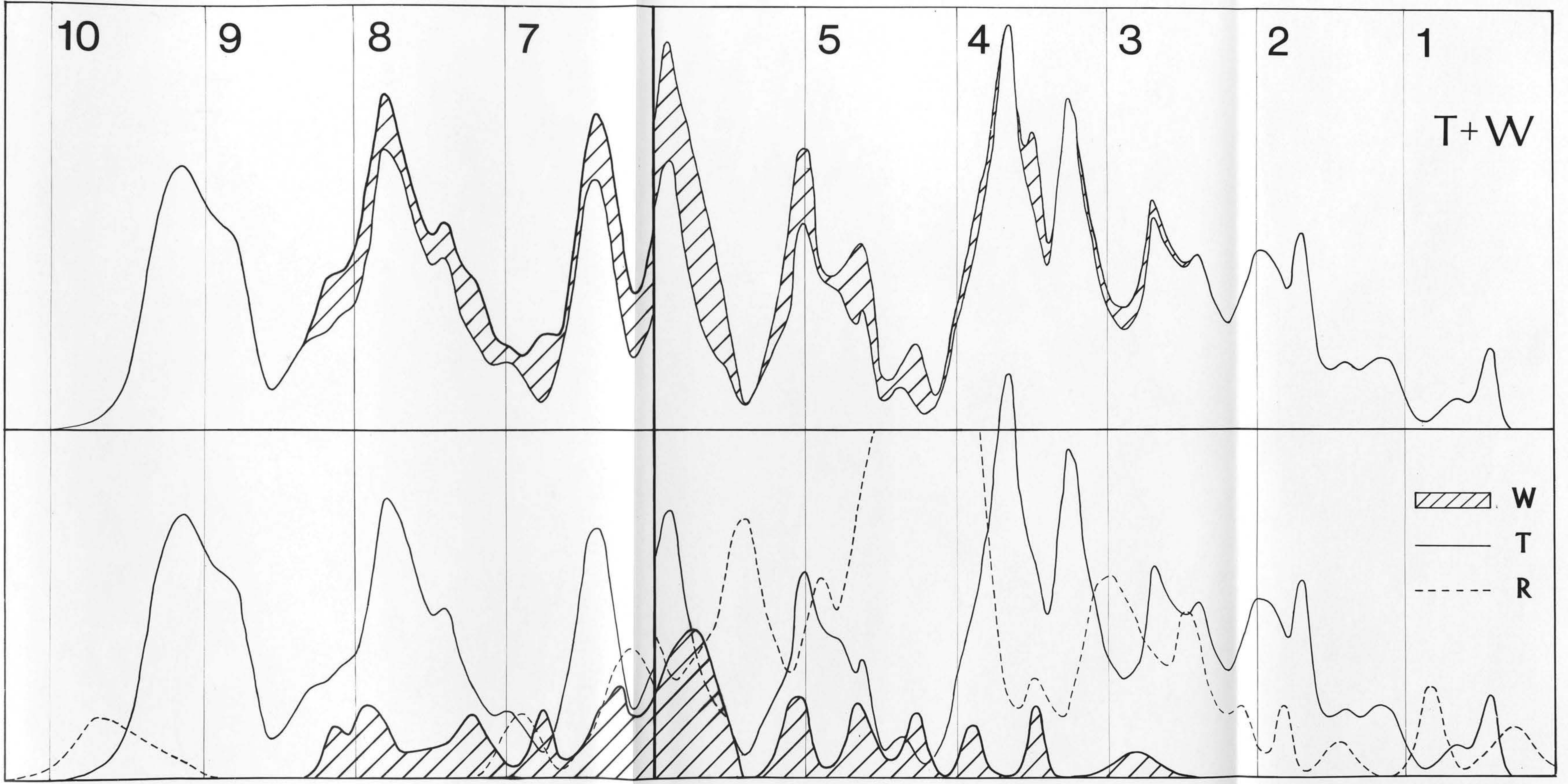

10
9
8
7
5
4
3
2
1
T+W
W
T
R

Figure 6.8 The curve summing the RI evidence inverted relative to that for the TW evidence.

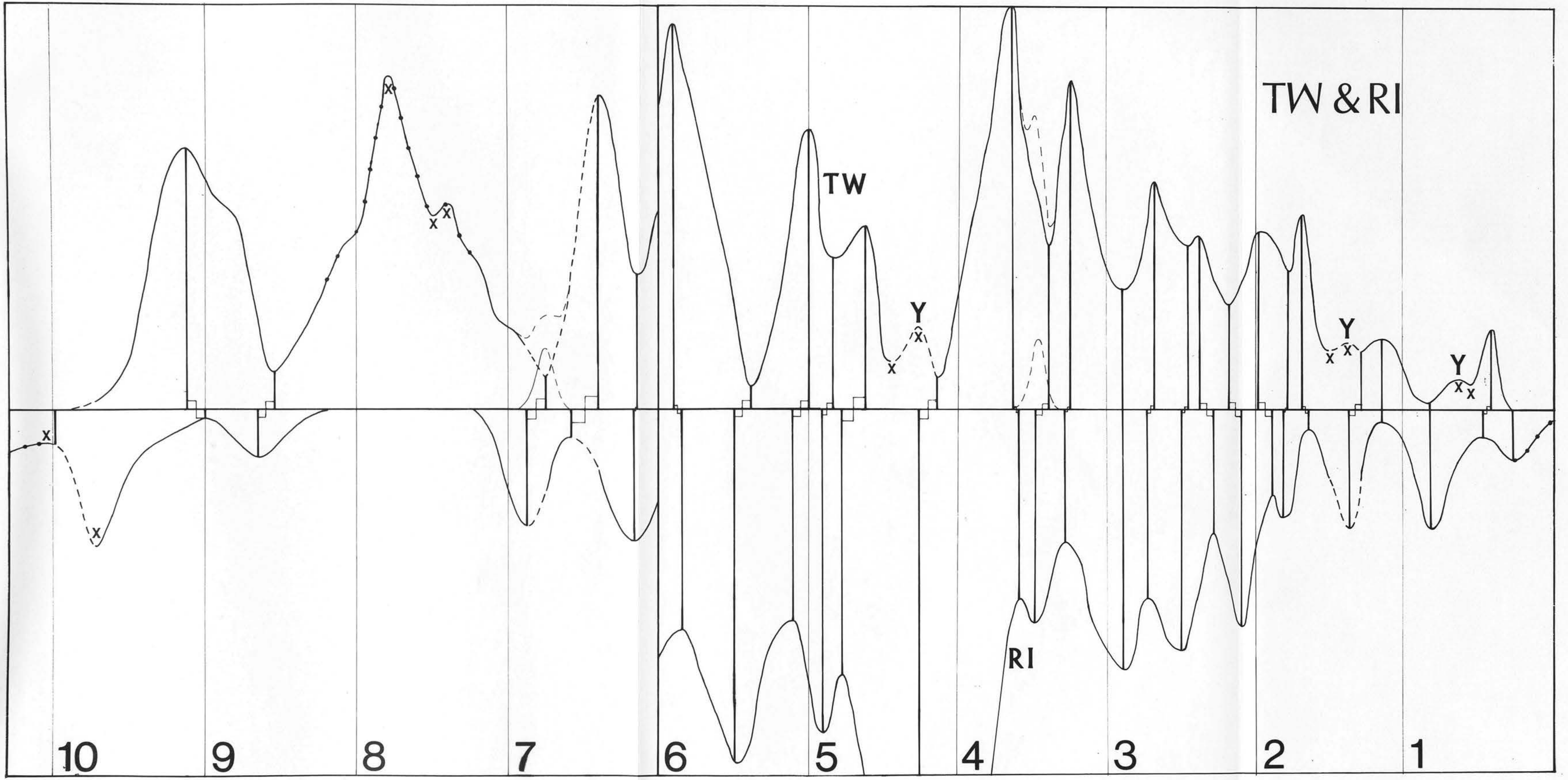
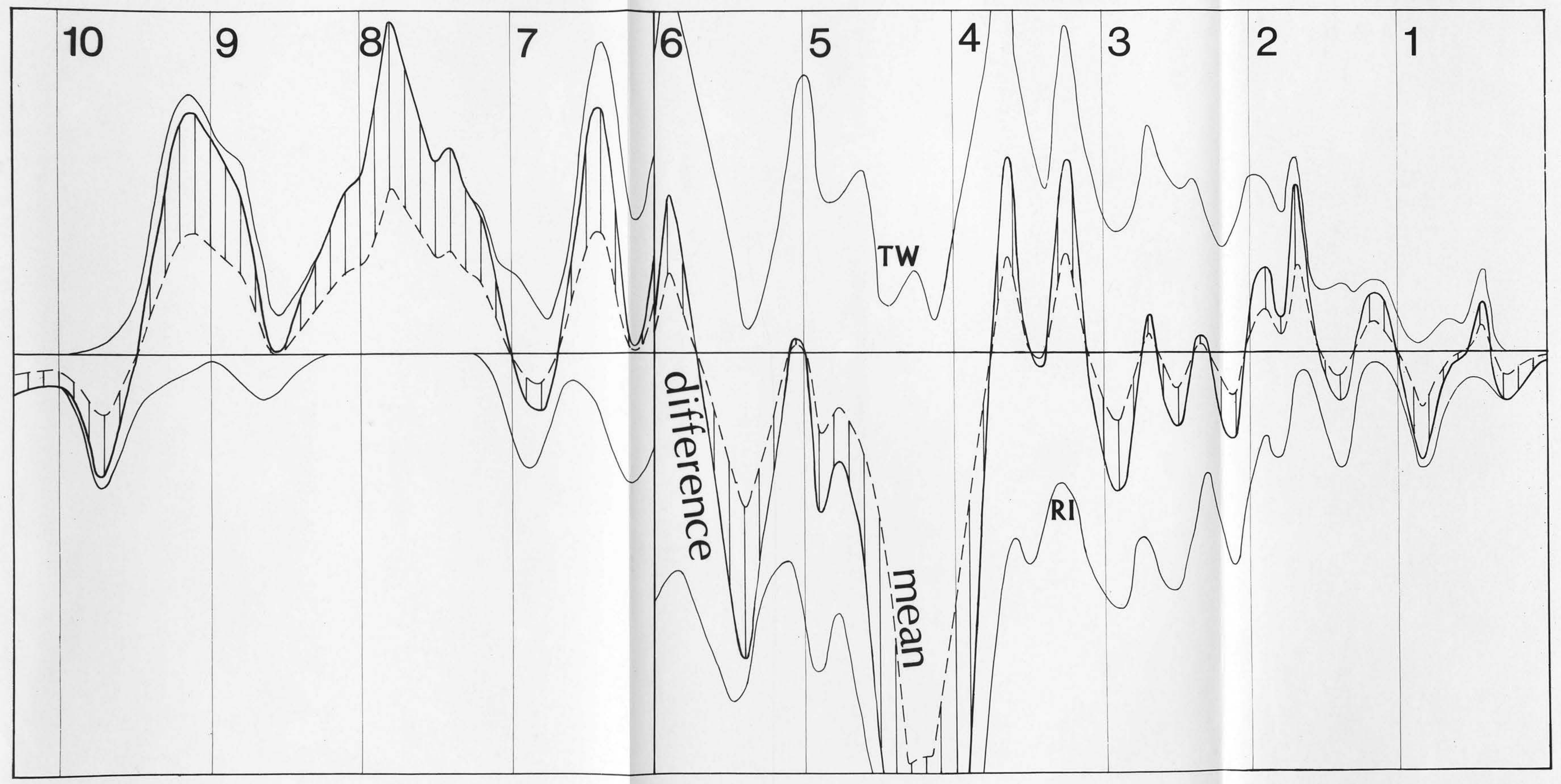

TW & RI
TW
RI
Y
Y
Y
x
10
9
8
7
6
5
4
3
2
1

Figure 6.9 Derived "Mean" and "Difference" curves.



10
9
8
7
6
5
4
3
2
1
TW
difference
mean
RI

Chapter 7

The special case of evidence from the Baltic prior to 6000 B.P.

i) Introduction

Evidence from the Baltic is given special consideration at this point for two reasons.

Firstly, to the best of the writer's knowledge, the approach used in the preceding section to define the initial hypothesis of sea level change has not been used before. It thus seemed advisable, before proceeding further, to make a check of its general geological validity by means of a pilot study of a well-documented region.

The Baltic in particular was selected from several possible such regions for a second and specific reason. This was that its connection with the oceans was intermittent during the earlier part of the Holocene. As will be indicated below, in several respects this presented particularly interesting opportunities for a check on the validity of the approach used here. It also made a special study of the timing of the breaks in the ocean connection advisable if Baltic evidence from the earlier part of the Holocene was to be admitted to the survey as a whole. Since well over a third (37%) of the available western European dates prior to 6000 B.P. refer to the Baltic, it seemed preferable to assess their relevance to the survey as a whole in some detail.

The chapter is divided into six sections. After the present Introduction (i), these are:

ii) The pattern of the Baltic evidence as a whole

iii) Published opinion on the dating of periods during which the Baltic was a lake

iv) Comparison of the patterns of radiocarbon dated evidence

(iv) cont'd.) from inside and outside the Baltic prior to 6000 B.P.

v) Discussion of the results of the comparison

vi) Conclusion

ii) The pattern of the Baltic evidence as a whole

In the approach used in Chapter 6 to frame the initial hypothesis of change, three of the four Classes of evidence used followed patterns that could be regarded as highly consistent in geological terms and the fourth, Class W, exhibited a pattern which appeared to be geologically reconcilable with the others. The possibility that the grouping apparent in the dates making up the four patterns might reflect merely extraneous fluctuation in radiocarbon level would appear to have been eliminated. However, the possibility remains that the apparent agreement in these patterns was spurious in other respects. The principles used in the selection or classification of the dates might be misleading in some unforeseen way, or in this particular application the statistical technique used in deriving the graphs might produce anomalies of types unsuspected by either Professor Thom or the writer.

It was therefore decided that it would be useful at this point to cease to restrict attention to the categories of evidence used in framing the initial hypothesis, and to make a pilot study to determine whether any geologically meaningful relationship existed between the patterns of these categories and those of the other main Classes. If the relationship between the various patterns appeared inconsistent in geological terms the nature of the inconsistencies might help to indicate the sources of error, while if the patterns showed a high level of compatibility the general validity of the approach would gain support.

The use of the Baltic for a pilot study of this nature was considered admissible because although the connection with the oceans was broken at certain stages, its own shores were affected throughout by a single unified water surface. Provided evidence from inside and outside the Baltic is not mixed, assumptions need not be made here regarding the timing or nature of those breaks, since only the internal consistency of the evidence is under consideration at this stage.

The patterns of the T, R, I, P, U and S classes of evidence prior to 6000 B.P. in the Baltic are shown in Figure 7.1. No Class W dates occur at this period in the Baltic. It will be recalled from Chapter 5 (iv) that Class M contains dates of miscellaneous implications, and their combined pattern is not therefore geologically meaningful. Consideration of this class is for this reason postponed until Chapter 8, below.

In the diagram, the graphs of Classes R and I are shown inverted (conforming with the practice in Chapter 6). Neither of these classes is well represented in this period. Class R is represented by only a few dates with large standard deviations prior to 8000 B.P., and after that the curve is broken for a period of about half a millennium, centred on 7000 B.P. The Class I curve does not start until 8700 B.P., and the Class remains poorly represented until ca. 7500 B.P. In view of these shortcomings, it is considered that little weight can be put on the fact that the relationship between the Class T and the Class I and R curves is less systematic than that found in Chapter 6, where more abundant evidence was under consideration. As the graph shows, much more evidence is available in Class T than in either of these Classes. This curve will therefore be used as the main criterion in the comparisons which follow.

Two groups of Class S (shell) dates are represented. The difficulty of interpreting the relationship between shell beds and transgressive or regressive phases was noted in Chapter 5. This is so in the case of the earliest group of dates, all of which come from the same shell beds (at Vajern, $58^{o}8^{o}$). This shows two maxima on the graph. The one prior to 10,000 B.P. refers to dates of shells from below an erosion layer in the shell bank (U 2029; U 2030); the peak at 9750 B.P. to shells from above that layer (U 582; U 589). It is not clear at the site whether the erosion was due to purely local causes, or whether the sets of dates refer to a change or changes in water level. The correspondence of the latter peak to a minor peak on the T-curve thus can not be evaluated.

In the case of the group of shell dates centres on 7800 B.P., the relationship is however unambiguous. These dates (U 565 and 566, again from Vajern) refer to shells which in stratigraphy form part of a transgressive sequence. As the graphs show, their maximum in the S-curve corresponds to a major peak of the T-curve.

The Class U dates represent submerged forests exposed underwater on the present sea bed. Geologically, the dates of material of this kind might be expected to reflect the end of a period of regression with the onset of the submergence and subsequent rise of sea level which killed the trees and preserved their remains. As will be seen from the graphs, the dates form two major groups, one between 9600 and 8800 B.P., peaking at 9300 B.P., the other between 8300 and 7600 B.P., peaking at 8050 B.P. Each of these conforms to geological expectation in that each starts in close association with a minimum of the T-curve,and then continues through the succeeding period dominates by dates of transgression rather than regression contacts.

Class P is the most plentifully represented category of evidence after Class T, among those examined in this pilot study. As in the cases of

Classes R and I, the graph of Class P is shown inverted. As noted in Chapter 5, like Classes T and R, this class is made up of dates of samples from intermarine peat beds, but unlike these classes, it is composed of the cases which cannot be classed as definite transgression or regression contacts. In some instances this classification is necessary merely because insufficient information has been published to justify inclusion in Classes T or R, but characteristically the Class P samples involved here were taken from the central parts of intermarine peat beds. It therefore seems reasonable, geologically, to assume that on balance this class may be expected to refer principally to the periods between transgressions; i.e. any pattern seems likely to follow a course that is broadly reciprocal to that of the Class T.

That this is in fact essentially the case may be seen from the graphs. The P-curve starts just before 10,000 B.P. and increases to an initial maximum of such evidence at ca. 9500 B.P. The T-curve had however begun to rise abruptly ca. 9600 B.P., and the amount of Class P evidence falls off to a minimum that corresponds with the maximum of Class T at ca. 9200 B.P. to within less than a century. As the T-curve then falls, the amount of Class P evidence increases to a maximum just before 9000 B.P., which in turn corresponds to within less than a century with the next minimum of Class T. Between ca.9000 B.P. and 8150 B.P., the T-curve shows a broad peak, with a flattened top, and the P-curve matches this with a wide trough. At 8150, both end in complete synchronism, the former with a minimum, the latter with a maximum. In the next millennium the general form of the curves again correspond but the timing is less precise. The agreement between the T-maximum and the P-minimum at ca.7100 B.P. is again within a century. After 6900 B.P. relatively little Class P evidence is available, and the curves cease to correspond closely.

It would thus seem from the series of comparisons made here that as a whole the patterns tend to be compatible, and that their relationships are of types which are consistent with the geological implications of the various types of evidence involved. That this applies to categories of evidence beyond those used above in the construction of the initial hypothesis suggests that the consistent pattern obtained when only Classes T, R, W and I were used in fact reflects geological changes rather than any unexpected side-effect of the procedure used.

iii) Published opinion on the dating of periods during which the Baltic was a lake

Before proceeding further, it is necessary to consider the problem of periods in which the water level in the Baltic behaved independently of eustatic changes. To avoid possible confusion because of this, in Chapter 6 Baltic evidence prior to 6000 B.P. was shown separately in the diagrams, and was not discussed along with the data from the remainder of the western seaboard of Europe. This was considered necessary at that stage because, as noted above, Baltic dates make up over a third of the evidence in that period and erroneous assumptions regarding the timing of the lake phases could thus have seriously affected the initial hypothesis of change in that period.

Now that that hypothesis has been arrived at independently, however, this substantial body of data may be reconsidered, with two ends in view. Firstly, an investigation of the changing pattern of the relationship between the watersurface outside and inside the Baltic itself offers a series of specific checks on the validity of the initial hypothesis of eustatically dominated change. Secondly, if the duration of the lake stages may be established with sufficient clarity, the information from Baltic marine phases prior to 6000 B.P. may be added to the material for consideration in the main analysis of the pattern of the western European evidence which follows.

In the present section, published opinion on the changes in the nature of the Baltic will be explored, then in (iv) the implications of the C^{14} date patterns will be considered.

Changes in the Baltic have been a subject of research since the 18th century, when Celsius and Linné made a distinguished beginning. The literature is extensive, and the basic sequence of "Baltic Ice Lake", "Yoldia Sea", "Ancylus Lake" and"Litorina Sea" is well established.

At the end of the last glaciation a freshwater lake was dammed up between the retreating ice cap and the European continent (Fig. 7.2). The history of this "Baltic Ice Lake" was complex, and though its main overflows were in the Oresund area and by various routes across Sweden, it may have connected with the White Sea at a relatively late stage (early Gothiglacial, e.g. Sauramo, 1947). However, since the classic work of Lundqvist (1921) it has been accepted that it was finally drained when the retreat of the ice opened a passage at Mount Billingen in south Sweden. This is the stage shown in Fig.7.2.

The drainage caused an abrupt drop in waterlevel of some 26 metres. This event has been widely identified in both Finland and Sweden, but it has not yet been the subject of direct radiocarbon dating. The Billingen drainage occurred at the very end of the Younger Dryas pollen zone, however, and this has been C^{14} dated locally at 10,000 - 9950 B.P. (T. Nilsson 1964; Berglund 1966; Morner 1969). E. Nilsson (1968) also dates the event by Swedish varve chronology to 10,163 B.P.

As indicated earlier, it is not yet possible to be certain of the relationship between varve and C^{14} chronologies. Until fuller correction data are available, instead of using the 5570 half-life it is probably most reasonable to base a comparison on the 5730 half-life, considered since 1962 the best available in absolute terms (Godwin, 1962; Kra in Radiocarbon, 1970). In either case, the degree of agreement between varve and C^{14} datings seems sufficient to justify some confidence that the Baltic Ice Lake terminated not later than ca. 9950 B.P. on the timescale used in the present study.

The drainage marked the beginning of a stage during which the Baltic was connected with the ocean by the so-called Narke Sound across central Sweden. Saltwater flowed into the basin, and the stage is generally known as the Yoldia Sea.

The continuing isostatic recovery of the land, freed from the weight of the decaying icecap, led to the progressive shallowing of the Narke Sound until it was reduced to the "Svea River" (S on Fig. 7.3) and a lake stage was re-established, the so-called "Ancylus Lake", (Fig. 7.4), before contact between ocean and Baltic was finally re-established via the col in the Danish Straits (Fig. 7.5)

It is now over a century since Schmidt (1869), working in Estonia, first drew attention to deposits of this lake stage, and over 80 years since Munthe (1887) gave impetus to its study by his work in Gotland. Unfortunately for present purposes however, both the lake's time limits and the relationship of its level to the ocean have yet to be definitively established.

The questions of whether it was of long or short duration, and of its overall placing in time have been the subject of long and continuing controversy. This variety of opinion has continued into the present decade, despite the increasing availability both of direct C^{14} dates of Ancylus material and of indirect dating through C^{14} dated pollen zones.

Views on the duration of the Ancylus lake period seem to fall into two main groups, favouring a short and long duration respectively. The shorter estimates average about half the length of the longer, though the extreme opinions are for durations bearing a ratio of 1:4. For instance, among the shorter group, Konigsson (1967) favours 600 to 1000 years, and Berglund (1964) 700 to 100 years; Sauramo (1954) however considers that the lake stage was as short as 500 years. On the other hand, Fromm (1963) considers a duration of some 2000 years, in line with his earlier proposal (1938). Similarly, Munthe (1955) expressed scepticism of Sauramo's view and maintained his own opinion of ca. 1700 years, an estimate he had argued at length in an earlier paper (1940) in which he quoted many workers with similar findings. Hyyppa independently criticised Sauramo's view, and made a case for a period of some 1500 years. (Hyyppa, 1963)

The placing of the period in time has centred about the millennium between 8000 and 9000 B.P., but even within the last decade, limits as early as ca. 9700 B.P. and as late as 7000 B.P. have been proposed. Fromm's 2000 year span (op.cit.) runs from 9000 to 7000 B.P., and Hyyppa's 1500 years (op.cit.) from 8500 to 7000 B.P. The earliest dating is that of Berglund (op.cit.), whose Ancylus period runs from ca. 9700 to ca. 8700 B.P. This does not overlap at all with Sauramo's estimate, nor with the similar one of Virkala (1966), or the ca. 8300 to 7400 B.P. period proposed by Kliewe (1960). It lies virtually end to end with the estimates by Lundqvist (1965) of ca. 8700 to 8000 B.P., by Kolp (1964, 1965) of ca. 8750 to 8100 B.P. and by Konigsson (1967) of ca. 8800 to 7800 B.P. The end of the lake stage proposed by the latter three, however, falls as long before Fromm and Hyyppa's estimate of the end of the period as these dates for the beginning lay after Berglund's proposed commencement. In each case the discrepance is the greater part of a millennium. The recent literature thus can not be said to offer a clear indication of the dating of the Ancylus Lake.

As will be indicated below, the development of the lake stage was different in different parts of the Baltic. The proposed timings described above show little suggestion, however, of any broad geographical pattern which might lead to a reconciliation of the conflicting views on grounds of the differing regional development of the lake stage. Rather is the contrary the case.

For example, both Hyyppa and Sauramo based their estimates on Finland, yet the durations of the periods they propose bear a ratio of 3:1. Similarly, Kolp and Kliewe drew their conclusions in the south Baltic from neighbouring areas (Usedom and the Mecklenburger Bucht respectively) yet their estimates together cover a span of some 1350 years, only 200 of which are in common between them. Furthermore, Berglund based his conclusions on Blekinge, located in south east Sweden near Oland, where Konigsson worked, yet their estimates together span as much as 1900 years, with only a single century of overlap.

The conflicts therefore appear to arise from genuinely divergent interpretations, rather than reconcilable regional variations. That this is likely is borne out by the nature of the evidence.

The literature as a whole makes it clear that even easily visible Ancylus lake shoreline features are morphologically similar to those of earlier and later marine phases. The problem of identifying the lake phase on morphological grounds is further exacerbated by the submergence and burial of the lake shorelines in the whole of the southern part of the Baltic due to continued isostatic tilting (e.g. Munthe, 1941, Lundqvist, 1965, inter alia), while in the northern Baltic the shoreline, though exposed, is very indistinct because of the rapidity of the isostatic uplift (e.g. Nansen, 1912). In Finland, Donner (1965) has emphasised that postglacial shorelines, including the Ancylus ones, can not be followed for long distances, and has drawn attention to the uncertainty of correlating shoreline remnants.

As with morphology, the lake clay itself can seldom be readily distinguished in the field from the preceding and succeeding postglacial marine clays of the Baltic. This is because it is often in fact reworked clay of the Yoldia Sea (Trask et al 1939 - Symposium on Recent Sediments) and has in turn contributed to later deposits.

Even detailed analysis of flora and fauna does not give a definitive answer. In the parts of the Baltic basin with deepwater sediments, saltwater diatoms occur throughout the period of interest (e.g. Strøm, 1957, 1961), while in contrast elsewhere, and particularly in the north (i.e. most distant from the sea connection) it is clear that the water was fresh even during the Yoldia Sea period (Donner, op.cit.). Even where changes certainly did occur, work by Heinonen (1957) has confirmed earlier findings by Backman and Cleve-Euler (1922) that point to the probability that interpretation of the lake episode may be complicated by the redeposition of relict diatoms washed out of earlier materials. Donner (op.cit.) underlines the difficulty of using pollen analysis to identify and date the Ancylus stage.

In general, the literature makes it clear that in the evidence of pollen, mollusca and diatoms, there is not a clearcut "salt: fresh: salt" alternation definitively identifying the Ancylus lake phase. Instead there has been a continuous variation in salinity, both geographically and in depth, throughout the Holocene history of the Baltic.

In view of the problematic nature of the evidence, it is perhaps hardly surprising that not only the timing of the Ancylus lake phase but also the relationship of the Ancylus lake level and the level of the oceans outside has remained a matter of controversy. For the present study, it would be valuable to be able to distinguish whether the level of the Baltic during the lake phase did or did not reflect eustatic changes. Both views are extensively represented in the Scandinavian literature. Many workers, from von Post, in 1924, to Lundqvist in 1965, have considered that the level of the lake lay between 15 and 32 metres above contemporary sea level. This opinion appears to have come closest to general acceptance.

However, the view that the lake was essentially at the level of the sea outside has gained persistent support from Antev's 1917 paper onwards, from amongst others De Geer (1922, 1925), Tanner (1930), Sauramo (1929, 1942) and Florin (1944, 1948). This school of thought considered that at least during some periods there was a direct connection between the lake and the ocean through the Danish Straits, and that the narrowness of this connection and the addition of glacial meltwater to the Baltic combined to keep the "lake" relatively fresh.

Although the actual date of the definitive termination of the Ancylus Lake stage remains in doubt, there is general agreement that there was no later lake stage and that the Baltic has remained ever since a branch of the ocean. The term "Litorina Sea" is sometimes used to cover the whole period from the termination of the Ancylus Lake to the present day. Subdivisions, on grounds of salinity, (Mastogloia substage; Litorina Sea proper; Limnaea and Mya substages) need not be considered here, since throughout this period it is generally accepted that eustatic sea level changes affected water level in the Baltic directly, through the Danish Straits.

In summary thus far, then, it would seem that evidence of Baltic level changes prior to 10,000 B.P. can be considered with some confidence to relate to the Baltic Ice Lake, and not to changes of ocean level. Baltic radiocarbon dates from this period are therefore irrelevant to the present study. The literature on the period between 10,000 B.P. and 7000 B.P. is characterised by controversy concerning both the timing of the Ancylus Lake stage and whether changes in the lake's surface level did or did not reflect changes in ocean level. In view of the magnitude of the unresolved conflicts in the Scandinavian literature, it seemed inadvisable to accept any of the views expressed there without first attempting to devise an independent, empirical approach to the problem.

iv) Comparison of the patterns of radiocarbon dated evidence from inside and outside the Baltic prior to 6000 B.P.

It was decided to make use of the same technique as that used in Chapter 6 for framing the initial hypothesis of sea level change, but to process the Baltic C^{14} dates of Class T, R and I samples completely separately from the dates from areas indisputably accessible to the direct influence of the oceans. Two sets of graphs were accordingly calculated, so that the patterns of events in western Europe inside and outside the Baltic could be compared. As shown above, the Ancylus Lake has been claimed to fall between extremes 2700 years apart (9700 and 7000 B.P.). The graphs were prepared to run from the beginning of the Holocene to 6000 B.P. Again using the same technique as in Chapter 6, these graphs were reduced to Difference Curves showing the overall trend in each area. These curves were then superimposed (Fig. 7.6).

It will be seen that from the start of the Holocene until just after 9600 the curves disagree. Between ca. 9800 and 9600 B.P. no clear evidence is available from the Baltic, but the non-Baltic curve begins to rise steeply after a peak of regression evidence just before 9700 B.P. It is joined in this rise after 9600 B.P. by the Baltic curve, and both reach a peak about 9200 B.P. (There is a high degree of agreement in the placing of this turning point - only ca. 80 years spread.)

Both graphs then fall, but whereas the non-Baltic curve steepens at about 8800 B.P. and eventually reaches a minimum between 8600 and 8550 B.P. The Baltic curve begins to level off about 9000, and in complete disagreement with the non-Baltic curve rises until 8700 B.P., when it levels off until about 8500 B.P. By then the non-Baltic curve is rising steeply, and the fall of the Baltic curve to a minimum between 8200 and 8100 B.P. is again in complete disagreement.

After 8100, however, both curves rise markedly to what is essentially an identical peak (only 20 years spread) at 7800 B.P. From this they fall together to identical minima ca. 7500. In the ensuing rise, the non-Baltic curve rapidly reaches a maximum at 7400 B.P., and then falls steadily. The Baltic curve however continues to rise until 7100 B.P., before also beginning to fall. It starts to rise again just before 6700. The other curve agrees very closely with this (to within 40 years), if the single Class W date already adjudged anomalous on other grounds in Chapter 6 is disregarded.

It is considered safe to do this, since one of the few things on which the literature is agreed with regard to the dating of the Baltic lake stages is that the marine connection was not broken after 7000 B.P. In accord with this, both curves rise from their minima of 6700 B.P. to agree closely at a peak just before 6400 (40 year spread), from whence they fall to minima at 6150 B.P. which agree exactly, before rising together towards a peak at about 5900.

In summary, then, the curves show marked agreement from 9600 to just after 9000 B.P., from 8100 to 7400 B.P., and from just before 7100 B.P. onwards. The periods of disagreement cover (at least) 500 years from the start of the graph, until just less than 800 years from ca. 8900 to 8100 B.P., and some 300 years from 7400 to 7100. Implications of this pattern will now be discussed.

v) Discussion of the results of the comparison

Prior to 9600 B.P., there is little clear C^{14} evidence that is relevant to the present topic. This is reflected in the closeness of the Baltic curve to the **abscissa** prior to ca. 9800, while between 9800 and 9600 B.P. the evidence is so sparse and contradictory that it cancels out. Little weight can therefore be placed on conclusions drawn from the graph in this period. It will be noted, however, that the disagreement in the Baltic and non-Baltic curves prior to 9800 B.P. conforms with what would be expected from the proposed dating of the Baltic Ice Lake.

The marked rise of the Baltic curve after 9600 B.P. refers to the event that Berglund (1964) considered to be the Ancylus transgression. This is clear from his graph of shore level change (1964, p.35). It was his view that in his field area in Blekinge (southeast Sweden) the shorelines of the Yoldia Sea probably lay below present sea level and that the transgression of Blekinge culminating about 9200 B.P. was that of the Lake. He notes however that the diatom content is somewhat different from the typical Ancylus assemblages described in central Sweden by Maj Brit Florin (1944, 1945, 1946) and Sten Florin, (1948). No other modern workers besides Berglund have placed the Ancylus period so early. Konigsson on Oland off the Swedish east coast certainly described peats which he had radiocarbon dated in the 9300 - 9000 B.P. range as being located "under the Ancylus beach", but this was a matter of stratigraphic description and as noted above he considered that the lake phase commenced ca.8800.

The closeness with which this peak of transgressive evidence in the Baltic coincides with the peak for the rest of the western European seaboard at 9200 B.P. suggests strongly that the level of the Baltic at that time reflected changes in the level of the oceans outside. The implication of the graphs is that the connection characteristic of the Yoldia Sea stage was still open, and probably remained so until at least 9000 B.P.

From 8900 to ca. 8150 B.P., the gradients of the graphs are in complete disagreement. The inference drawn is that in this ca. 750 year period the surface of the Baltic behaved differently from the seas outside. Whereas the evidence of transgression outside the Baltic falls to zero about 8600 - 8550 B.P. before increasing during the rest of the millennium, in contrast in the Baltic the evidence of transgression increases from 8900 to 8700 B.P., then maintains a protracted maximum until 8500 B.P. before falling to zero about 8200 B.P. It therefore seems probable that this represents the main Ancylus Lake stage.

This empirically-based conclusion accords well with recent results from Scandinavia. For instance, Morner (1969) reports extensive work in the Viskan valley, which runs inland from the Kattegat , near the Baltic to Kattegat connection of the Yoldia Sea period at Svea. It is interesting that if his ALV2 shoreline is extrapolated to Svea, it falls at + 107 m, i.e. just 2 m above the Baltic to Kattegat col. On the basis of a considerable programme of radiocarbon dates, he dates the drying out of the ALV2 shoreline at 8850 B.P. This implies that the connection was broken at a time very close indeed to that suggested by the graphs derived above (i.e. 8900 B.P.) Comprehensive studies of diatoms in Baltic Sweden by Hornsten (1964) also appear to support this. If the conclusions stated by Vogel at the 1969 Nobel Symposium on Radiocarbon Dating concerning the relationship between C^{14} and absolute ages during this millennium are accepted, and Hornsten's varve datings are recalculated so that the points to which they correspond on the 5570 half-life C^{14} timescale used here may be established, the agreement seems very close indeed. On the scale used here on the graphs, his diatom samples show that distinctly salt water conditions terminated at 8900 B.P., and a sample at 8500 B.P. showed distinctly fresh water.

The decline of transgressive evidence in the Baltic after 8500 B.P. shown on the graph (Fig. 7.6) may also have a specific geological meaning. After the sea connection at Svea was broken, the strait there became a river with extensive rapids (Svea Alv) carrying overflowing fresh water from the Baltic Lake. This condition persisted until the continuing glacio-isostatic tilting of Sweden lifted the threshold at Svea above cols to the south at Oresund and Darsser Schwelle in the Danish Straits. As long ago as 1928, Von Post's classic paper established that the Svea River dried out in terms of pollen chronology at the rise of the alder curve, and the validity of this conclusion is now firmly accepted (e.g. Freden, 1967). Lundqvist (1957) radiocarbon dated the rise of the alder to 8450 B.P., and T. Nilsson (1964) confirmed this for Scania, at ca. 8550 B.P.

The fall of the graph after 8500 B.P. would therefore appear to correspond directly with the acquisition of new exits by the Baltic Lake. In this light the evidence of Gotland is interesting. There the Ancylus shoreline is better developed as a morphological feature than, in general, further north (where isostatic uplift was more rapid) or south (where over wide areas it is buried). In Gotland Lundqvist (1965) considers that the highest limit of the Ancylus lake (the "Ancylusgransen" or AG) dates from 8550 B.P. Donner, who had earlier (1966) considered that the Ancylus transgression maximum in Finland and Estonia was as early as 8950 - 8750 B.P., has recently (1969) accepted the view of Kessel and Raukas (1967) that it fell later, at some point in a 500 year range starting 8450 B.P., though he points out that it still can not be fixed with precision.

It seems that when the Danish straits supplanted the Svea Alv, ocean level was still somewhat lower than the level of the Baltic. T. Nilsson's conclusion of 1935 that the level of the Baltic fell at least 4 m because of erosion of the thresholds at Darsser Schwelle has remained unchallenged, and has recently been reinforced by Berglund (1964) who showed a regression of 7 m in Blekinge. According to the graphs presented above, the curves for the Baltic and the western European coasts beyond the Baltic cease to conflict between 8200 and 8100 B.P., and then rise strongly to very closely coincident peaks at 7800 B.P. The inference would seem to be that the marine transgression which had been becoming steadily more apparent outside the Baltic from ca. 8550 onwards reached the thresholds in the Danish straits about 8200 B.P. and that for at least the next 800 years, during which the curves agree closely (i.e. until 7400 B.P.) the Baltic was once more an arm of the ocean. Krog's results (in Morner, 1969) from the Store Baelt of Denmark do not appear to support this, but they are difficult to asses, since his evidence has not yet been fully published.

Krog did not apparently encounter an influx of saltwater at this period. Such an influx is however evident within the Baltic itself, for example in the Mecklenburger Bucht of north Germany (Kolp et al 1965), and in southeast Sweden where Berglund (1964) placed the onset of brackish conditions at ca. 8200. Morner has recently suggested (1969) that this apparent discrepancy was due to salt reaction currents following the deep channels in the Danish straits and not affecting Krog's shallower sites. This certainly accords with comments by Donner (1965) on the depth distribution of diatom evidence.

On balance thus far then, the pattern exhibited by the graphs appears to be meaningful in geological terms, and it seems reasonable to conclude that the disagreement in that pattern between ca. 8900 and 8200 B.P. does in fact represent a period during which the water level of the Baltic behaved in a different way from the level of the sea affecting the remainder of the western seaboard of Europe. This period would therefore seem likely to be the Ancylus Lake stage.

The diversity of the published estimates of the length and timing of the period was indicated above. Nevertheless, the majority of these views seem broadly compatible with the interpretation derived empirically here from the radiocarbon dated data.

The present estimate overlaps substantially with those of Kolp (1964, 1965), 8750 - 8100 B.P.; Lundqvist (1965) 8700 - 8000 B.P.; Konigsson (1967), 8800 - 7800 B.P.; Virkala (1966), 8600 - 8000 B.P.; Sauramo (1958), 8500 - 8000 B.P., as well as the older estimate by Sten Florin (1944), 8900 - 7900 B.P.

The only major opinions with which the estimate derived here is in conflict are those of Berglund (1964, 1966) and Kliewe (1960). These overlap slightly with the ends of the span defined here (though not with each other), but they are centred on periods some 750 years earlier and later, respectively. Berglund's interpretation has already been discussed, and Kliewe's estimate lacks radiocarbon datings.

The present estimate also overlaps substantially with the earlier parts of the more recent long-duration estimates, by Fromm (1963), 9000 - 7000 B.P., and Hyyppa (1963) 8500 - 7000 B.P. The latter parts of these estimates will be considered further at a later stage below.

The existence of a phase of low salinity has long been recognised between the end of the Ancylus Lake stage proper and the sharp increase in salinity marking the onset of the conditions characteristic of the Litorina Sea. Some writers (notably Granlund 1943) have recognised this phase but have not distinguished it by a special title. Many however follow Munthe (1910,1940) in referring to this low salinity substage of the Litorina Sea as the "Mastogloia Sea" (equated with the so-called "Clypeus Sea" of others, for instance Lundqvist, 1965). Many writers place the onset of Litorina Sea conditions proper around 7000 B.P. Some radiocarbon dates of material attributed to the Litorina phase fall earlier, but as far as could be ascertained the usage of "Litorina" merely implies "post-Ancylus", and no distinction is implied in these cases between "Mastogloia" and "Litorina" per se. With two exceptions (which presumably represent erroneous attributions since they fall before the bulk of dates which refer to material specifically identified by the submitters of the samples as Ancylus), all these earlier dates fall after the curves resume their common pattern at 8200 B.P.

The earlier of these dates include Su 61 8200 ± 270; Su 60 7950 ± 190; Su 37 7835 ± 170; St 1616 7815 ± 90; St 1590 7770 ± 190; St 1583 7750 ± 90. The only dates falling substantially within the later period of disagreement of the curves between 7400 and 7100 B.P. all appear to refer to peat later transgressed by the Litorina Sea and not to the Sea itself. (Those whose ranges fall most completely within this period are St 1588 7220 ± 95, and St 1613 7260 ± 100.)

The curves resume their agreement ca. 7100 B.P., and this coincides with the start of a remarkably well-defined grouping of dates firmly attributed to the Litorina phase. These include:

Mo 223 7180 ± 270; St 1618 7125 ± 95; Su 28 7110 ± 170;

Mo 224 7110 ± 170; U 43 7110 ± 130 ; Su 63 7100 ± 170;

TA 181 7085 ± 80; Su 32 7080 ± 140; U 42 7070 ± 140;
St 1609 7025 ± 180; St 1622 7005 ± 140; Su 16 7000 ± 180;
U 41 6960 ± 130; Su 41 6870 ± 165.

This is the earliest large grouping of dates attributed to the Litorina Sea, and its importance is emphasised by the nature of many of the attributions.

For instance, Mo 223, Su 28, U 43, U 42, Su 16 and U 41 are all characterised specifically as referring to the oldest or highest Litorina shorelines identified by the workers submitting the samples, while Su 32 is defined as representing the beginning of the Litorina period proper. All these attributions are supported by pollen and/or diatom analyses. That the date grouping is of more than local importance in the Baltic is attested to by the distribution of the dates: Su 16 and 28, Karelia U.S.S.R.; Su 41, 59, 62, 63, South Finland; Su 32, West Finland; TA 181 and Mo 223, Estonian S.S.R.; Mo 224, Latvian S.S.R.; U 42, 42, 43, Sodermanland, Sweden; St 1609, 1618, 1622, Gotland Island.

In some of these cases however (Gotland, South Finland) it is specified that this was not the first identifiable Litorina event. The scatter of dates, back to 8200 B.P. quoted above, supports this in general terms, again in both the east and the west Baltic. The title "Litorina I" has in fact been used specifically in referring to samples as early as 8200 B.P., i.e. over a thousand years earlier than the main group of radiocarbon samples attributed to shoreline sites under this classification.

This might be taken to imply that individual Litorina shorelines are time transgressive to a major extent. This interpretation would however appear to be unjustified.

The close consistency in timing of the main group of "Litorina I" attributions centred on 7100 - 7000 B.P. considered in relation to both the widespread nature and the general pattern of their geographical distribution would necessitate complex and improbable hypotheses of land movement if these dates were to be linked with others up to a millennium earlier in terms of time transgressive shorelines. In no case however does any shoreline appear to have been followed on the ground between sites providing contrasting dates. The attribution to "Litorina I" is consistently an interpretation, based in turn at best on an interpretation of diatom evidence. The general problems of distinguishing between different phases of the Baltic and the shortcomings for this purpose even of diatom data have already been indicated.

That the thousand-year range of the dates classed as "Litorina I" is more likely to represent miscorrelations of scattered evidence and inconsistent use of terminology than a widely time transgressive shoreline is emphasised by the fact that at several of the radiocarbon dated sites more than one "post-Ancylus Lake" feature or deposit may be distinguished in the period ending with the date group at ca. 7000 B.P.

This may be illustrated for instance by the sequences at the sites at Dynisse, Gotland, and Porvoo, South Finland. At the former, the Litorina sequence is represented by five dates, ranging from the earliest at 7815 ± 90 (St 1616) to those at the main grouping, 7125 ± 45 and 7025 ± 180 (St 1618 and 1609 respectively). At Porvoo, "Litorina I" was considered to begin at 8200 ± 270 (Su 61) and reach its maximum about 7950 ± 190 (Su 60). The dates falling in the grouping around 7000 B.P. were therefore classified as "Litorina II" (Su 63 7100 ± 170; Su 41 6870 ± 165; Su 62 6810 ± 165), by Hyyppa (1966), although as shown above sites giving radiocarbon dates in this group in neighbouring areas to the east, south and west were classed as "Litorina I".

On balance, then, adequate reconcilation of the attributions in terms of a time transgressive shoreline appears unlikely.

The pattern of the Difference Curves suggests an alternative explanation. It will be recalled that these follow the same pattern from 8200 to 7400 B.P., then conflict for three hundred years before again conforming from 7100 B.P. onwards. On this basis it may be suggested that between 8200 and 7400 B.P. the level of the Baltic came under the influence of sea level. As noted above, however, it is generally agreed that the phase immediately following the Ancylus Lake stage was one characterised by a relatively low salinity and sometimes referred to as the Mastogloia Sea substage. The discrepancy in the curves between 7400 and 7100 B.P. would seem to suggest that this brackish phase is unlikely to have terminated definitively until after 7100. This is in fact supported by many diatom diagrams (e.g. discussed by Berglund, 1964, Morner, 1969, inter alia) as well as by the strong grouping around 7000 B.P. of radiocarbon dates attributed to the initiation of Litorina conditions. However, there seems no clear indication in the literature as to whether the discrepancy in the curves indicates a definite break in contact between the Baltic and the oceans. The diatom histograms for instance offer neither definitive support nor clear denial because of the shortness of the period.

The persistence in the literature of long-duration estimates of the Ancylus Lake stage is however interesting in this context. A great many workers, from Munthe in 1910 to the present decade, have considered that evidence of lake conditions could be identified over periods of up to 2000 years. As noted above, the latest representatives of this viewpoint are Fromm (1963) and Hyyppa (1963), and the estimate of the timing of the main lake stage based on the Difference Curves overlaps with the earlier parts of their estimates. In the

1963 papers, they both placed the termination of the lake conditions at 7000 B.P. (though Hyyppa was forced to revise this by his "Litorina I and II" radiocarbon dates quoted above). The discrepancy in the Difference Curves between 7400 and 7100 B.P., as well as between 8900 and 8200 B.P. suggests that they and the earlier writers may have been correct in identifying lake conditions at periods ranging in all over almost two millennia, though incorrect in assuming that the periods formed part of a continuous whole.

Be that as it may, both the pattern of the curves and the evidence of individual radiocarbon dates seems consistent with the view that although marine influence was restored in the Baltic about 8200 B.P., and thus has been equated by some with the onset of Litorina conditions, it seems better to regard the ensuing period of relatively low salinity as the Mastogloia Sea substage and accept the majority usage that "Litorina I" refers to the period immediately following the last disagreement in the curves apparent between 7400 - 7100 B.P., since from then on both the literature and the graphs indicate unambiguously that the Baltic remained an arm of the sea.

In summary, then, it is concluded that prior to 9800 B.P. the Baltic and non-Baltic curves disagree and this probably reflects the existence of the "Baltic Ice Lake". Until 9600 B.P. Baltic evidence is sparse, but from then until ca. 8900 the curves are in marked agreement. This would appear to be the period of the "Yoldia Sea". From 8900 to 8200 - 8150 B.P. the curves show a marked conflict, which would appear to represent the main "Ancylus Lake" stage. The curves then agree until 7400 B.P., and it is suggested that this period is regarded as that of the "Mastogloia Sea" substage of the "Litorina Sea", and that the onset of the "Litorina" period proper is regarded as following the resumption of agreement between the curves at 7100 B.P.

vi) Conclusion

As was indicated in the Introduction to this section, the evidence from the Baltic prior to 6000 B.P. is given special consideration for two reasons. Firstly, as a check on the general validity of the methods used, and secondly to identify periods in which data from the Baltic are not relevant to the survey as a whole.

The Baltic evidence appears to confirm the validity of the approach adopted in the survey in several respects.

That the strong pattern evident in the Class T R I and W dates considered in Chapter 6 might merely reflect some unsuspected anomaly in the way the data were classified or represented had to be considered, despite the consistency of that pattern with the geological nature of the particular classes of evidence involved (i.e. those classes, T and W, indicating an increase of marine influence tended to agree in pattern, as did those, R and I, indicating a decrease; the periods dominated by the pairs of classes alternated strictly in time). In Section (ii) above, it was shown from the Baltic evidence that other categories of dates, besides those used in framing the initial hypothesis, also exhibited patterns that were consistent with the geological nature of the various types of evidence involved, and that, furthermore, the variations in all classes conformed to a coherent and geologically meaningful overall pattern. This would seem to render it distinctly unlikely that the patterns revealed by the techniques used here are spurious.

Other regions than the Baltic could have been used to demonstrate the geological consistency of the variations in the different classes of evidence. At an early stage the writer in fact ran a series of such checks in other areas to make sure it would be profitable to continue further with this particular method of analysis. Furthermore, as will be shown in the next chapter, even when the diverse coastal regions of western Europe are considered together, the graphs of Class S, P and U dates tend to show geologically consistent relationships with the TW and RI curves. The Baltic is, however, unique in western Europe in another aspect of its potential for testing the geological validity of the approach used.

The particular advantage of the Baltic as a test case for the general geological validity of the approach used in this survey lies in its intermittent connection with the oceans in this period. As the discussion of the "Litorina I" attributions shows, miscorrelations of shorelines can occur. In that case, many of the features involved fell in a particularly well-defined grouping in time, and followed a geographical distribution that extended across a wide range of land movement isobases. It was not difficult, therefore, to establish that the dates that diverged widely from that grouping were likely to belong to distinct features reflecting other events, rather than to a time transgressive shoreline. However, when dates occur in looser groupings in time, or when their geographical distribution is less clearcut in its implications, it can be difficult to make an adequate assessment of such possibilities. Thus if the episodes successively making and breaking the contact between the Baltic and the oceans had not occurred, it would have been difficult to secure a positive check of the geological validity of the methods used.

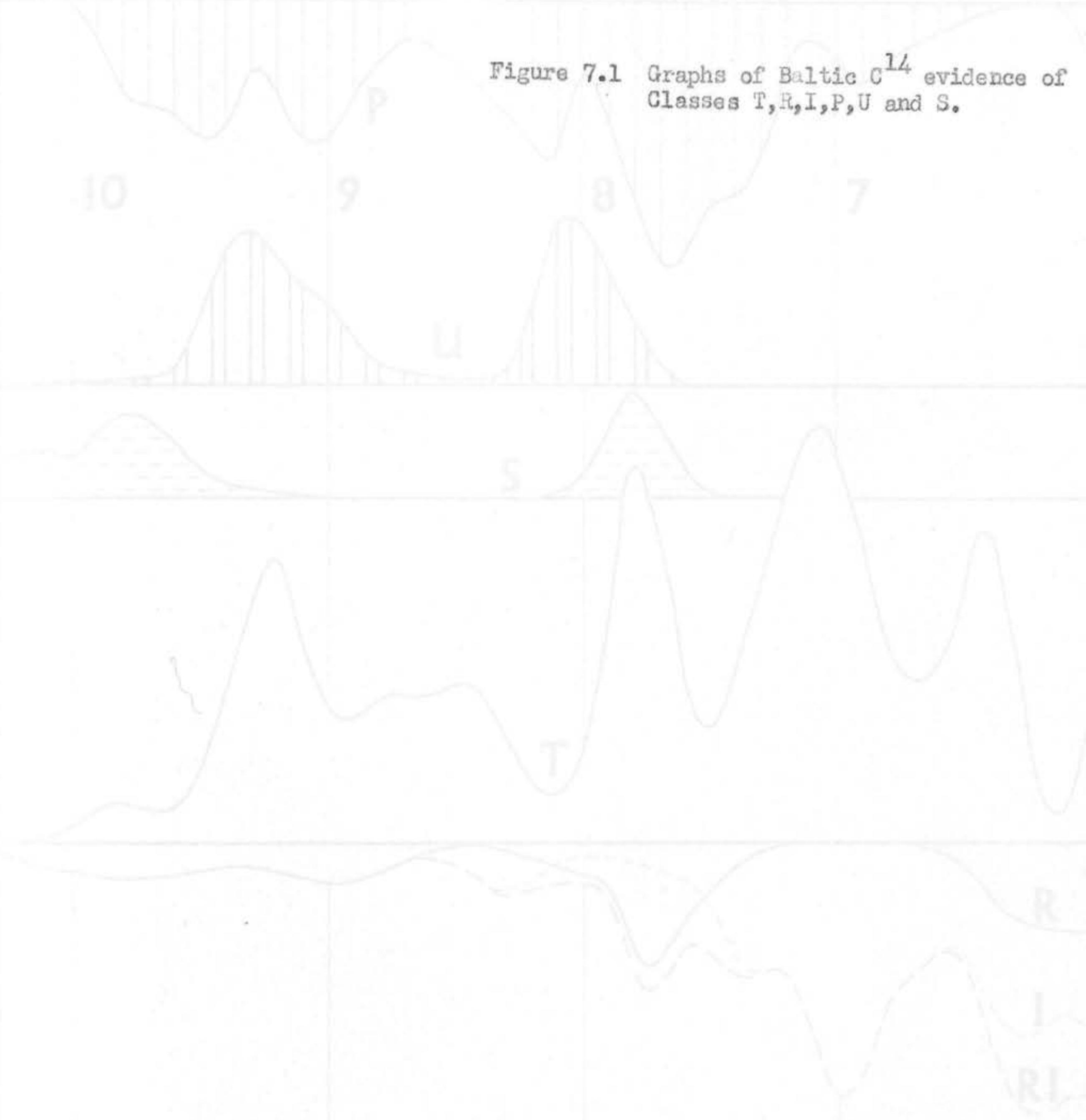
The difficulty of diagnosing the Baltic lake phases in terms of the nature of the deposits and the appearance of the morphological features has been emphasised, and the confused state of the literature arising from this has been indicated. Although the specific identification of lake phases on the ground at individual sites is problematic, it seemed reasonable to suppose that if the methods used here were valid, a comparison in terms of these methods of the sequences of transgressions and regressions represented inside and outside the Baltic should reveal differences attributable to the isolation of the Baltic from the sea outside.

If the curves representing the Baltic evidence had had no clear form, firm conclusions would not have been possible. If they had disagreed with the non-Baltic curves throughout the 4300 years of the test period, doubt would have been cast upon the validity of the methods, since diatom evidence makes it clear that phases of oceanic connection certainly did occur during this period.

However, by showing a clear alternation between phases in which the curves successively contrast markedly and agree closely, the comparison suggests that graphs do indeed reflect the intermittent isolation of the Baltic from marine influence, and that the approach used in the present survey is valid. This conclusion would appear to be reinforced by the way in which the definition of lake stages achieved using these methods appears to be compatible with the majority of recent opinions in the Scandinavian literature, and indeed to offer ways of reconciling some apparently conflicting views.

The second reason for making a special study of the timing of the breaks in the oceanic connection was to establish which radiocarbon dates from the Baltic area might be considered relevant to the remainder of the study, and which should be omitted from this. As noted above, before 9800 B.P. the curves disagree, probably because of the "Baltic Ice Lake" stage. Until 9600 B.P. the status of the Baltic is in doubt. Between 8900 and about 8200 B.P. the main "Ancylus Lake" stage appears to be in progress, and further interference with the marine link seems evident between 7400 and 7100 B.P. In the phases intervening between these periods, however, the pattern of the Baltic evidence conforms closely to that of the seaboard outside, and tends to accentuate the pattern if added to it (Fig. 7.7). It seems prudent however to extend the limits of the potential lake periods slightly, and to admit to the general corpus of data only those Baltic dates falling between 9600 and 9000 B.P., between 8100 B.P and 7500 B.P., and from 7000 B.P. through to the present day. This involves the rejection of 61 or the 140 Baltic dates occurring between 10,300 and 6000 B.P. It seems reasonably safe to assume that the remaining 79 dates represent periods when the Baltic was susceptible to marine influence.

Figure 7.1 Graphs of Baltic C^{14} evidence of Classes T,R,I,P,U and S.



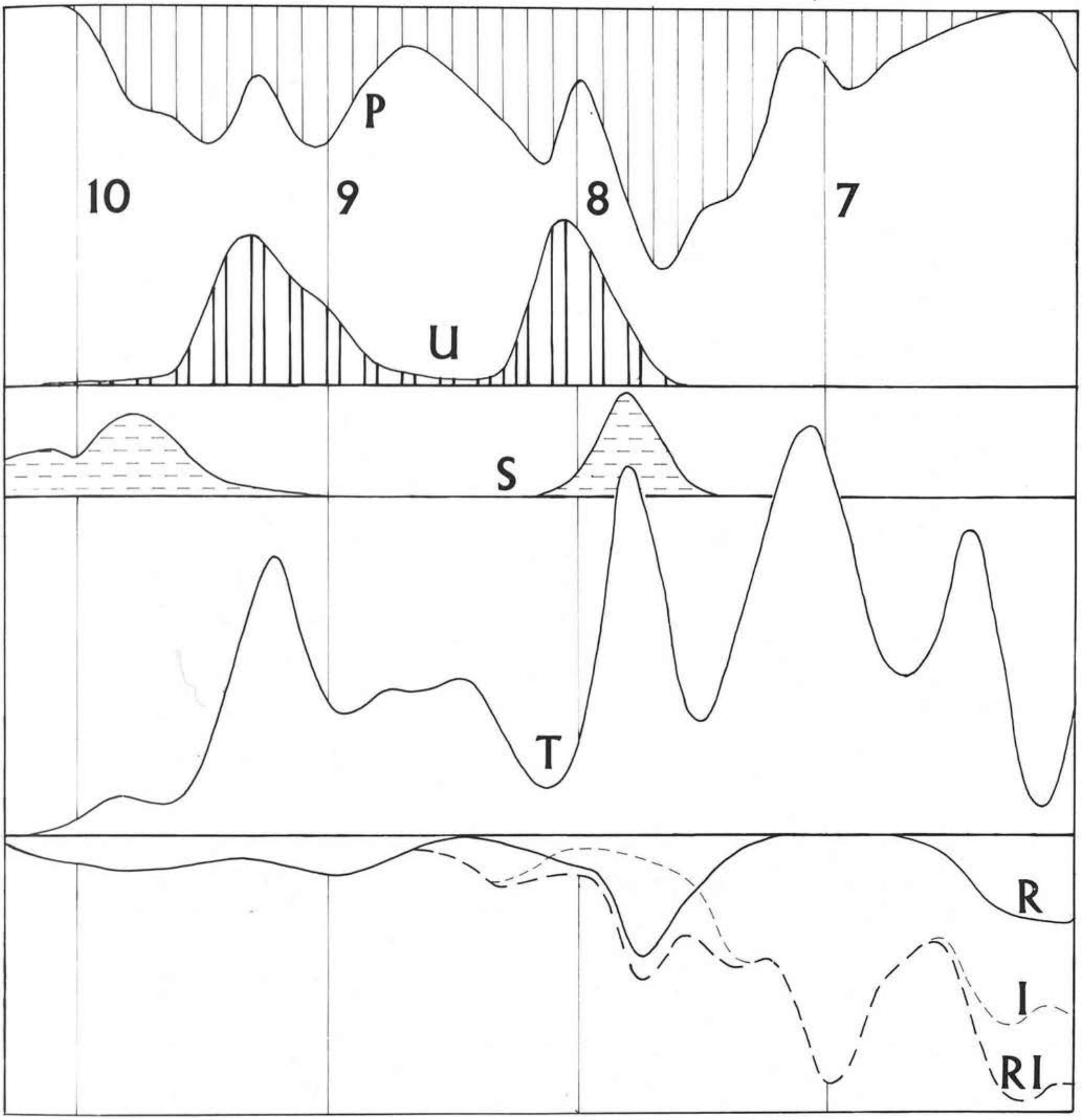

P
10
9
8
7
U
S
T
R
I
RI

Figure 7.2 Map of Baltic Ice Lake - after
Stenberger 1964, Fig.4, p.26.

Fig. 4. The 'Baltic Ice Lake' about 8000 B.C. before it was opened to the west; the southern Baltic and a large part of the bottom of the North Sea were then above sea level. (After Granlund.)

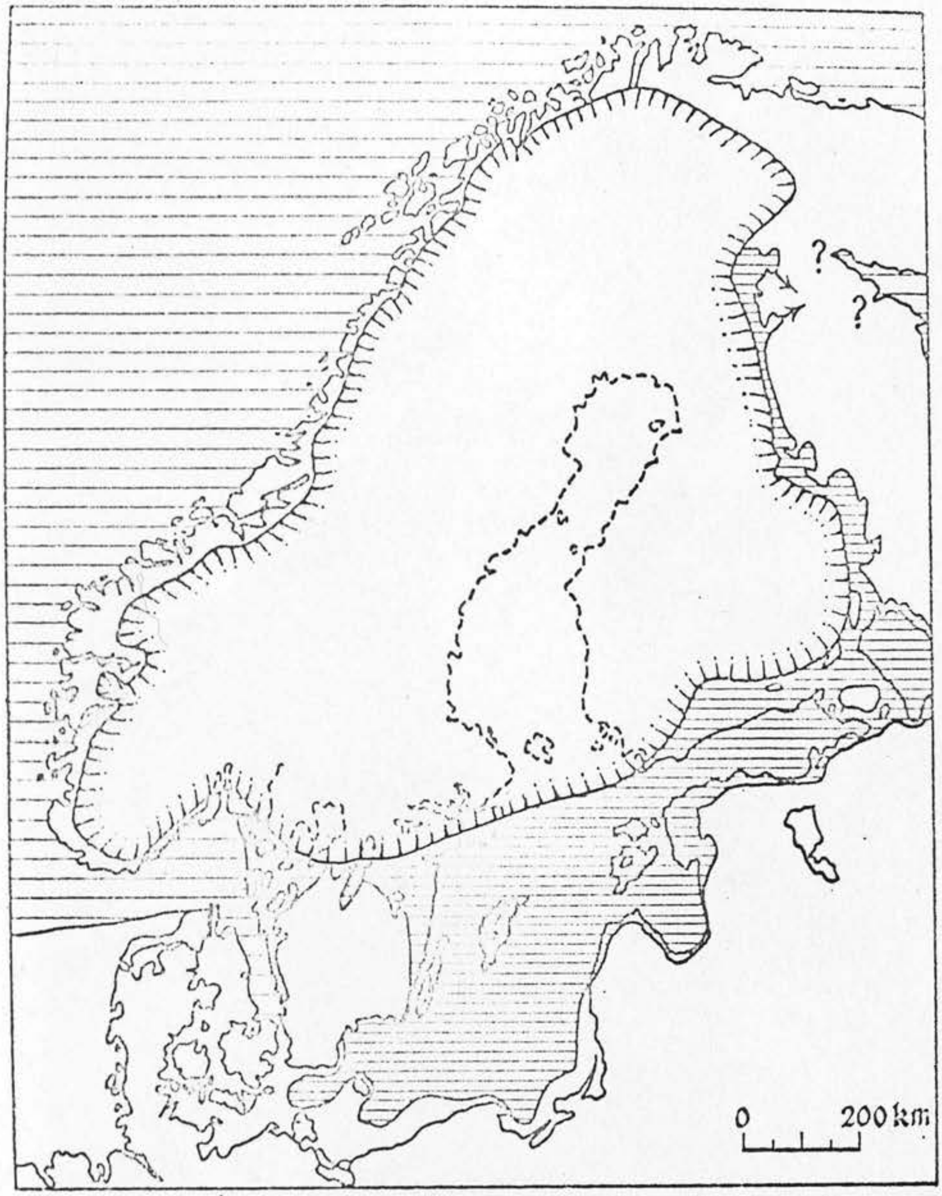


Fig. 4. The 'Baltic Ice Lake' about 8000 B C before it was opened to the west; the southern Baltic and a large part of the bottom of the North Sea were then above sea level. (After Granlund)

Figure 7.3 Map of Yoldia Sea - after
Stenberger 1964, Fig.5, p.27.

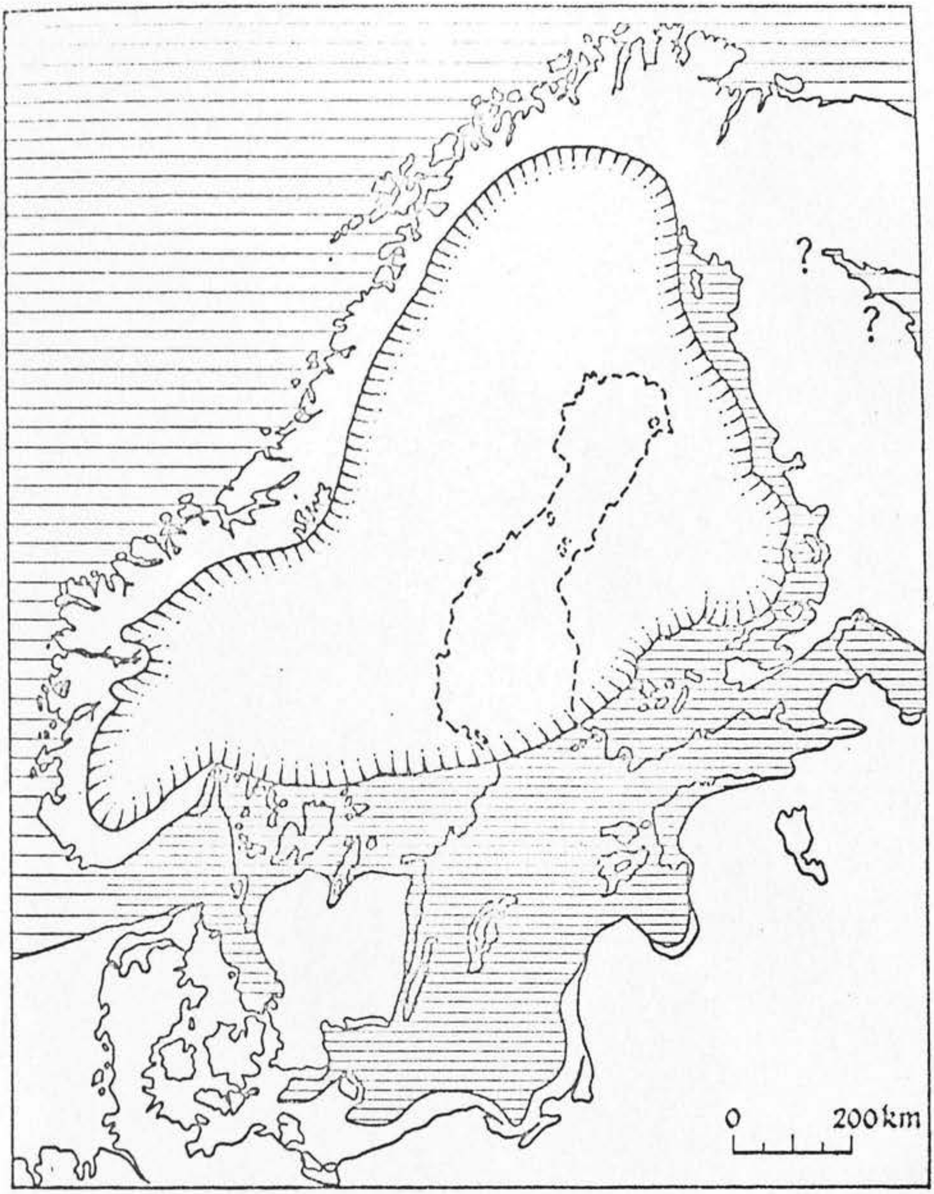


Fig. 5. The 'Yoldia Sea'. About 7500 B C the Baltic became a bay of the Atlantic, though much of the North Sea bottom was still above water. Southern Sweden was still connected to Denmark. (After Granlund and Fromm)

Figure 7.4 Map of Ancylus Lake - after
Stenberger 1964, Fig.6, p.28.

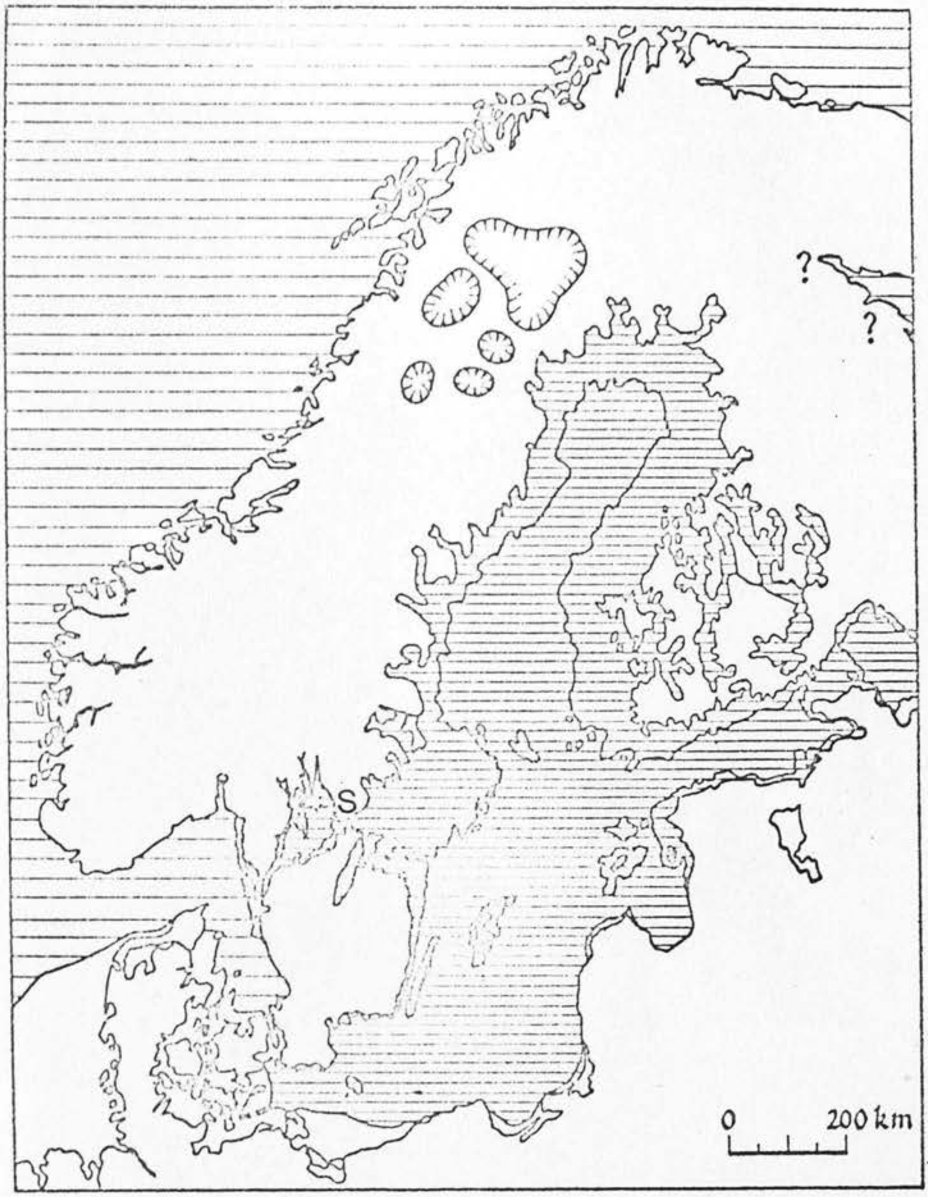


Fig. 6. The 'Ancylus Sea' about 6500 B.C. The Baltic was a freshwater lake with its surface 50–200 metres above present levels, flowing out through the present Göta Älv and the Danish Great Belt into the North Sea. Southern Sweden was still joined to Denmark, and the inland ice had almost disappeared. (After Fromm)

Figure 7.5 Map of Littorina Sea - after
Stenberger, 1964, Fig.7,p.29.

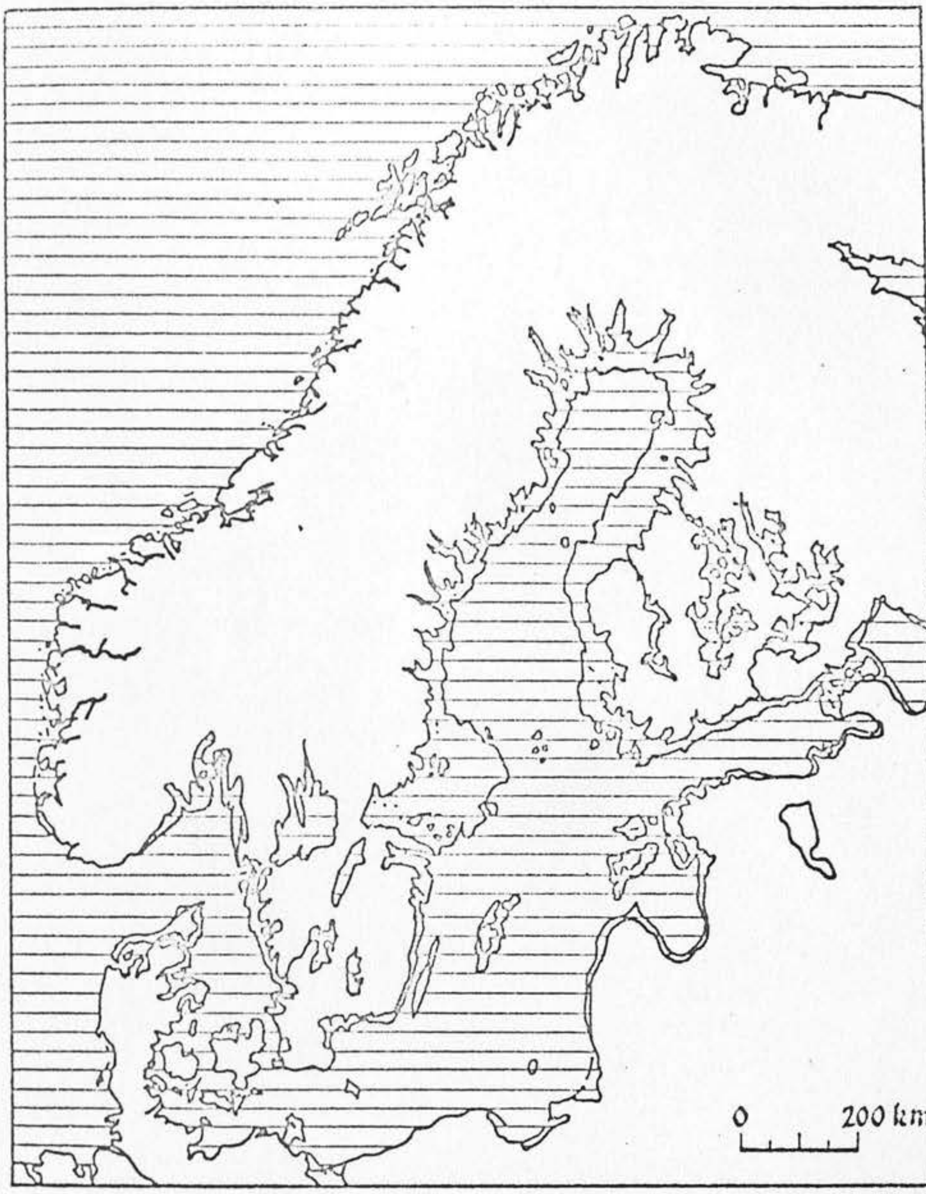


Fig. 7. The 'Litorina Sea' about 5000 B C. Öresund and the Danish sounds had formed, and the Baltic was directly connected with the Atlantic, its waters becoming salt again. A large part of the present coast of Sweden and Finland was under water. (After Granlund)

Figure 7.6 Baltic and Non-Baltic Difference Curves compared and superimposed.

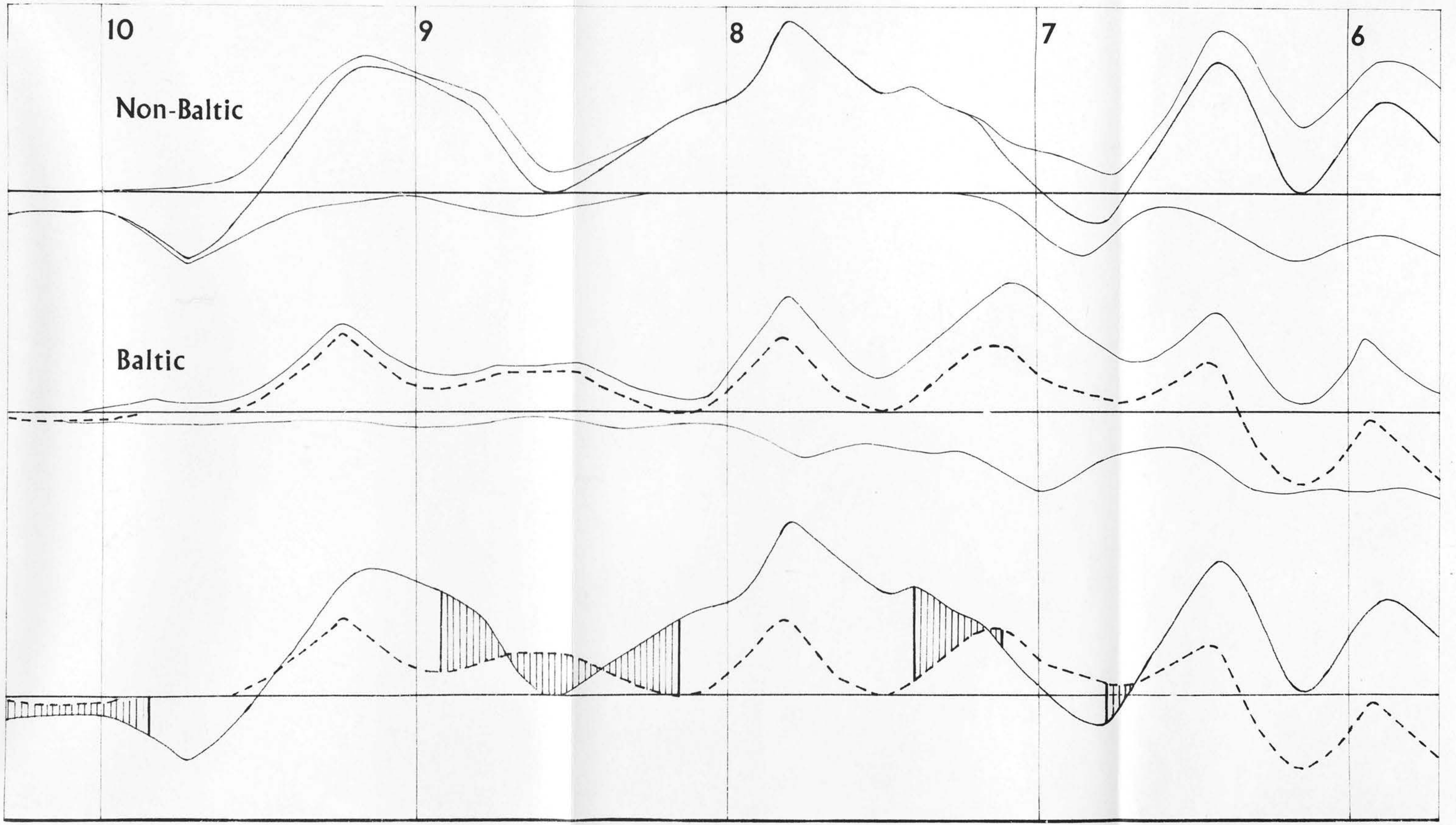
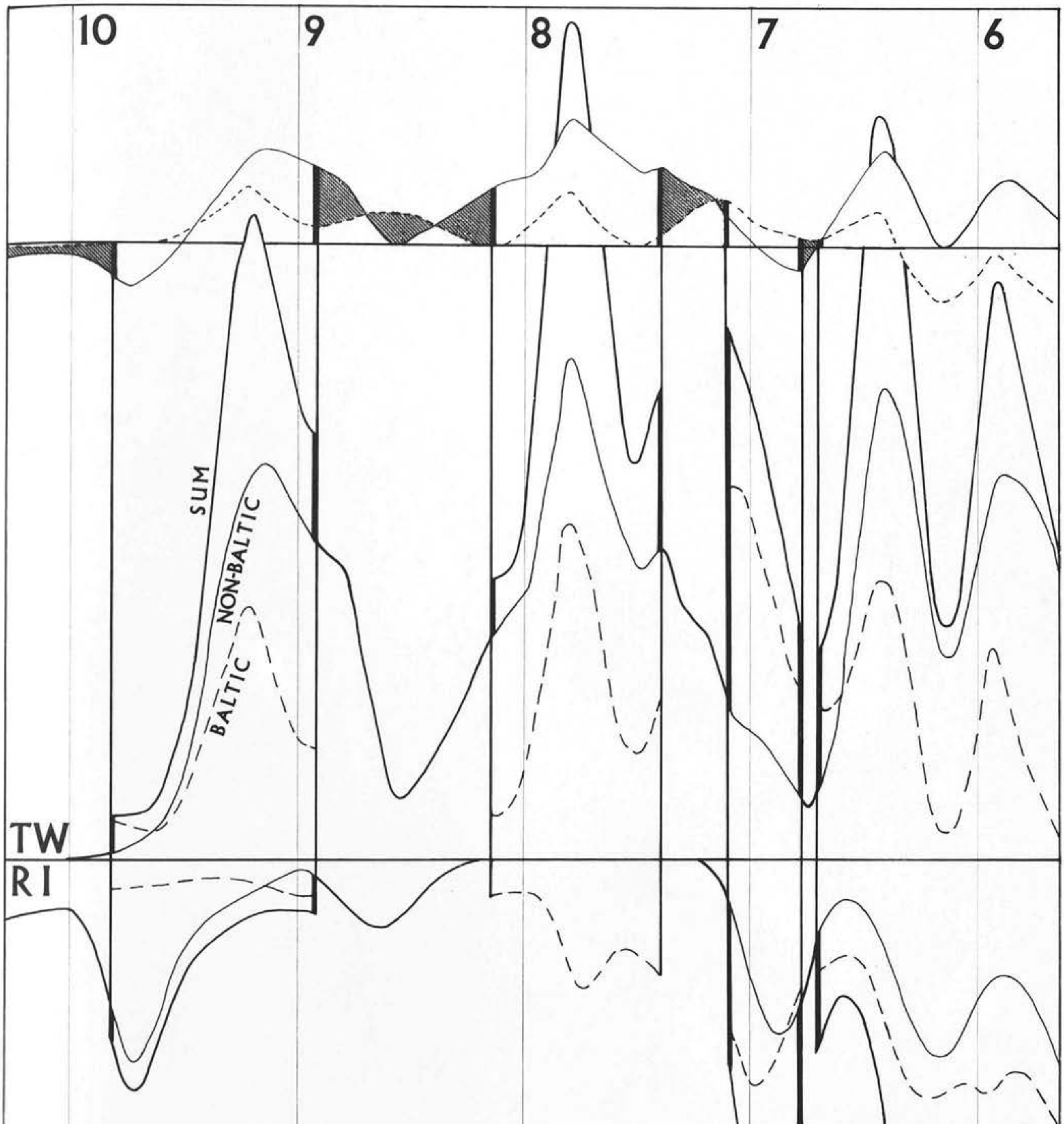

10
9
8
7
6
Non-Baltic
Baltic

Figure 7.7 Summation of Baltic C^{14} evidence <u>not</u> involved in potential lake stages, with the evidence from the remainder of the western seaboard of Europe.


10
9
8
7
6
SUM
NON-BALTIC
BALTIC
TW
RI

Chapter 8

The overall pattern of the radiocarbon dated evidence from the western seaboard of Europe, excluding Scotland

Part I: 10300 to 6000 B.P.

Introduction to Chapters 8, 9 and 10

In Chapter 6, an initial hypothesis of the sequence of Holocene changes on the western seaboard of Europe was derived on the basis of the 435 radiocarbon dates that seemed most reliable as indicators of changes in marine influence. In Chapter 7, the general validity of the approach used was examined, and essentially confirmed, in terms of the special case of the early Baltic evidence.

In Chapters 8, 9 and 10, the dates from beyond Scotland (ca.860 in all) will be considered as a whole. The validity of each phase in the hypothetical sequence will be examined in turn.

In Chapter 11, the results of this examination will be summarised, and the likelihood of eustatic control assessed in each case. In contrast to Chapters 8, 9 and 10, where the emphasis is on the time sequence and stratigraphy of the radiocarbon determinations, in Chapter 11 the assessment will be made primarily in terms of the map distribution of the dated sites, considered along with the range of conditions of land movement and coastal environment over which each event identified in the preceding chapters is represented. The coordinates of the distributions plotted there are given as each phase is documented and examined in the present three chapters.

Then, in Chapter 12, the pattern of Holocene eustatic change will be discussed, in terms both of the data from the present survey and of studies published by other workers, before assessing the available Scottish evidence on relative sea level change in the final chapters.

In Chapter 6, the initial hypothesis was derived on the basis of a technique that produced an objective graphical description of the way in which selected categories of dates were grouped in time. In Chapters 8, 9 and 10, an examination will be made of how far the patterns exhibited by other classes of evidence are compatible with the conclusions drawn from that grouping. In determining the level of validity of the different phases proposed in the initial hypothesis, special reference will also be made to those sites where a sequence of events, rather than a single episode, is dated.

It is necessary to consider the succession of events at individual sites because of the possibility that a single peak on the graph may in fact be made up from several stratigraphically distinct events. The classification procedure (Chapter 5) was designed to ensure that the categories T, R, W and I, used in formulating the initial hypothesis in Chapter 6 each consisted of dates with the same stratigraphic implications. However, at that initial stage no account was taken of whether individual sites contributed one or more such dates to each peak in the graph of its class. The possibility thus exists that in some cases a rapid succession of events might be stratigraphically distinct at individual sites, yet blurred into a single peak on the graph by "noise" effects. It is clearly advisable to investigate this specifically for each phase of the hypothetical sequence indicated by the graphs. The same is true of the converse, the possibility that some of the groupings of dates which appear as distinct peaks on the graphs may not in fact be represented by distinct features or strata on the ground.

Ideally, it would be feasible to resolve both these points completely from the sequences at the individual sites. In practice, however, this is not possible.

Even if all identifiable events had been dated at each of the sites included in the survey, it would be impossible in most cases to be entirely sure that the absence at some sites of evidence of an event which was represented elsewhere implied that that event had not in fact affected the whole seaboard. Local conditions might merely have prevented that event from registering clearly in particular areas, or have led to later destruction of the evidence there.

By no means every identifiable event has, however, been dated at the sites from which radiocarbon data are available. The cost of the dating process is such that even at sites where an extensive sequence with material suitable for radiocarbon determinations is available, only a limited number of dates is generally obtained. For instance, even at a particularly informative site with, say, four peat beds each separating marine sediments, it is unusual for more than three or four radiocarbon determinations to be available although as many as eight distinct changes in marine influence may be identifiable there.

The placing of these dated samples within the sequence reflects compromises made by the fieldworker. These are often made on grounds different from those central to the present study. A particular stratigraphic level may for instance be dated primarily because it offers a precise check of the local date of a pollen zone boundary, although secondarily it may give some general indication of the date of a local coastal change. Evidence of this weaker type was excluded from the Classes T R W and I, which were used in defining the initial hypothesis of change, but it must be examined here if the fullest possible picture of the sequence at individual sites is to be obtained.

Limitations such as these are however offset by the fact that at over two hundred individual sites, the timing of the stratigraphic sequence is defined by two or more radiocarbon determinations. Because of the shortcomings of the data, it seems inadvisable to place weight on absences of evidence. It seems preferable to concentrate on identifying positive disagreements between the number of transgressions and regressions implied by the graphs and those evident at individual sites. The large number of multiple-date sites suggests that some confidence may be felt in the effectiveness of this approach for bringing to light shortcomings in the initial conclusions that were drawn from the graphs.

In the discussions which follow, the evidence from both single-date and multiple-date sites will be taken into account, and the compatibility of all classes of evidence will be assessed.

For convenience, the description and discussion of the evidence will be split into three parts, dealing respectively with the early, middle and late Holocene. Since the survey involves the study of a continuum, rather than completely discrete events, some overlap between these sectors is necessary. Each will therefore deal nominally with the evidence of approximately four millennia, i.e.:

Part I 10300 - 6000 B.P. (circa 40% of the total of C^{14} dates)

Part II 7000 - 3000 B.P. (circa 50% of the total of C^{14} dates)

Part III 4000 - present (circa 30% of the total of C^{14} dates)

This form of organisation has been used primarily to prevent undue separation of subsections of the text dealing with different aspects of the same events, in what is necessarily a somewhat lengthy examination of the evidence.

In the interests of conciseness, in Parts II and III (Chapters 9 and 10, respectively), considerable use has been made of classified lists in describing the data. To illustrate the nature of the material and the criteria used in arriving at such lists, in Part I the data have been described in greater detail. For example, where a large number of dates in a given period all refer to, say, evidence of transgression at their sites, in Part I the diagnostic characteristic of the stratigraphy at each individual site is indicated, whereas in Parts II and III such dates are simply classified and identified in a list. Furthermore, to avoid repetition, as far as possible, explanatory comment on the handling of the material has been given in Part I in terms of the entire Holocene.

It is necessary to emphasise that these differences in presentation were effected only after the research was complete, and that they do not reflect a difference in handling of the earlier and later data.

PART I: 10300 to 6000 B.P.

In Section (i) of this Part, the phases of transgression represented by the Class TWRI graphs are identified. Then the relationships between the overall patterns of these classes and those of the other main categories of evidence (Classes S, P and U) are examined in (ii). The date sequences at multiple-date sites will then be considered (iii). The Baltic evidence will be set out separately first because of the problem of intermittent contact with the sea (iv), then the non-Baltic sequences will be listed (v), before the evidence as a whole is discussed in detail phase by phase (vi to ix). The conclusions are summarised, along with those from Parts II and III, at the beginning of Chapter 11.

Throughout Parts I, II and III, whenever authors are quoted without a specific reference being given to the literature, this indicates that the relevant reference is the entry in the journal Radiocarbon describing the date in question, and is thus specified in the Radiocarbon Date Appendix.

(i) The graphs used in formulating the initial hypothesis suggested five transgressive phases prior to 6000 B.P. These are identified in terms of the Difference Curve in Fig. 8.1 and illustrated more fully in Fig. 8.2.

Phase A can not be considered to be reliably defined by the Class TWRI graphs. It may begin sometime before the curves commence at 10300 B.P., and end as evidence of regression builds up between 10000 and 9750 B.P. This possibility is however suggested only by a minimum in the Class R evidence, and no positive Class T or W evidence of marine transgressions is available then.

Phase B on the contrary is clearly represented by a substantial peak of Class T evidence. From the Mean and Difference Curves, it starts at 9750 B.P., reaches its peak at 9150 and ends at 8550 B.P.

Phase C is equally well marked by positive evidence. Starting at

8550 B.P., it reaches its peak at 7800 B.P. The peak at 7400 B.P. is identified on the diagram (Fig. 8.1) as the maximum of Phase D, but it is a much less marked feature of the graphs than the peaks at 9150, 7800 or 6400 B.P. (or indeed any of the peaks F to P considered later). Thus on the basis of the graphs Phase C might well be considered to continue to 6800 B.P. However, at this stage it seems preferable to suspend judgement, and investigate all inflexions of the key curve impartially. Phase C is therefore defined as ending at 7500 B.P., and Phase D at 6800 B.P.

Phase E begins then and reaches a clear peak of positive evidence at 6400 B.P. before being replaced at 6150 by Phase F. This lasts until 5400 B.P. and thus lies outside the scope of the present section.

In general, then, in this period, evidence indicating increasing marine influence forms three substantial peaks on the graphs (B, C, E), together with one minor peak (D). Whether D is distinct from C or not, the period in which it falls is certainly characterised by positive evidence of transgression. In this it contrasts with the possible transgressive phase, A, which rests only on negative evidence when these graphs are considered alone.

With the exception of the earliest centuries of the graphs, where A lies, it is notable that evidence of transgression greatly outweighs that suggesting fall of relative sea level. Indeed, neither the Mean nor the Difference Curve descends below the abscissa between 9500 and 7000 B.P. (although both touch it at 8550 B.P.), while for a millennium during Phases C and D no Class R or I evidence whatsoever is available outside the Baltic. This imbalance contrasts markedly with the situation in the later parts of the Holocene, and its implications will be discussed at the end of the chapter.

(ii) Before the overall pattern of the evidence from prior to 6000 B.P. is discussed in terms of Phases A to E, it is necessary to discuss two points, one affecting Class S and the other Class P (iia and iib).

(iia) The overall pattern of the evidence is shown on Fig. 8.2. In the same manner as in Fig. 7.7, the Baltic evidence is added to the non-Baltic curves only in those periods in which it may be considered to reflect marine influence. The presentation is similar to that used in examining the overall pattern of the Baltic evidence, with one exception. This is in the representation of the Class S (shell) evidence.

In the Baltic chapter, relatively little shell evidence was available, and it was all plotted in the same way (i.e. shown above the Class S abscissa). Dates from those exceptional shell beds which can be proved to reflect local increases in marine influence are still shown this way. However, in Fig. 8.2, the curve representing all other shell beds dates is shown inverted, under the same axis. Some of these other dates were reported specifically as representing local falls in relative sea level, but the great majority were not clearly associated with either conditions of transgression or regression by those who collected the samples. The difficulty of making such attributions in a reliable way at individual sites has already been indicated. In the course of plotting the Class S curves, however, it became clear that throughout the Holocene those shell dates clearly indicating transgression tended to coincide with minima in the general curve of Class S evidence. Furthermore, the maxima of the general S curve tended to coincide with maxima of regression evidence represented in the Class R and I curves, while the minima tended to correspond to transgression maxima shown by the Class T and W curves.

It thus seems that the majority of shell beds that could not be identified in the field with marine transgressions do in fact represent conditions of decreasing rather than increasing marine influence.

(iib) The other general point that must be made before examining the pattern shown in Fig. 8.2 concerns the relationship of the Class P graph to the others. It will be recalled that this category contains those peat samples which could not be regarded as first-class evidence of transgression or regression, either because of inadequate publication or because of the nature of the sample itself.

Some for example came from the middle of relatively thick beds of peat, others from very thin layers. Even though each lay between marine sediments, only limited conclusions are possible when such dates are regarded individually.

A date from the interior of a thick intermarine peat bed may only show that that site was subject to a reduced marine influence at that time, without indicating when the regression and subsequent transgression took place. A thin bed may date from part of a regressive phase, or it may owe its growth and preservation to the rise of watertable at the onset of transgression. Indeed, as comprehensive studies by the Dutch (in particular) have shown, even a thick freshwater peat bed can indicate an increase of marine influence rather than a regression, because of the stimulus to rapid peat growth given by the backing up of freshwater drainage and the raising of watertables caused by a rising sea level. (Vermeer-Louman, 1936; Bennema, 1954; Jelgersma, 1961; Zonneveld, 1959). Thus a botanical assay of a peat that shows freshwater conditions need not necessarily equate that sample with a period of reduced marine influence. Similarly, indications of saltmarsh conditions in a thin peat may indicate either that that sample dates from the initial colonisation of its site on the withdrawal of the sea or that it reflects the onset of a transgression.

Although in the cases included in Class P, insufficient data were available from the sample sites for a satisfactory attribution to be made on an individual basis, as shown in the chapter dealing with the Baltic, the overall pattern of the Class P curve shows a strong enough relationship to the graphs of the more informative classes of evidence (T W R I) to suggest that for the majority of the period considered there, the peat samples concerned tended to correspond to periods between transgressions, rather than to phases characterised by rising relative sea levels.

It will be recalled that because of this broadly reciprocal relationship to the Class T curve, in the Baltic chapter the Class P graph was shown inverted relative to the T curve. This has again been done here, and it will be seen that the generalisation is still as valid outside the Baltic. A maximum of the Class P curve for outside the Baltic corresponds to the regression between transgressions A and B, then the non-Baltic Class P curve reaches zero for the period of the maximum of transgression peak B. After a maximum of Class P evidence corresponding to the regression period between B and C, the P-curve again reaches zero during the next Class T peak, C. The problematic minor peak D is not marked by an inflexion in the P-curve, but this does show a broad maximum corresponding to the minimum of transgressive evidence between C/D and E.

In the last half millennium of the period under consideration here, however, the relationship between the Class P and T W R I curves begins to change. The most marked peak of Class P evidence prior to 6000 B.P. corresponds not to a minimum of the Class T curve, but to a maximum. Its maximum coincides with that of the peak E to within a century.

On the basis of Fig. 8.2 alone, this might be thought to have little significance. The P-maximum at E might be considered simply anomalous, or it might be regarded as corresponding to the nearby regression maximum between E and E. However, when the evidence of the Holocene is regarded as a whole, it becomes apparent that the balance of the relationship changes at this point. Whereas prior to about 6500 B.P., both in the Baltic and on the western European seaboard as a whole, the Class P curve has been dominated by samples referrable to regression periods, from then onwards the curve tends to relate instead to the peaks on the T W curve.

This is illustrated by Fig. 8.3, which shows the T W, R I and P curves from 7000 B.P. to the present. All are shown in the same orientation, i.e. rising above the abscissa. Since Class P is made up of evidence that is less specifically diagnostic of sea level change than the evidence admitted to Classes T W R and I, the relationship is not precise. Nevertheless, it does appear to exist.

The first two maxima of the P-curve (1 and 2) correspond to within a century with maxima E and F of the T W curve. The intervening P and T W minima are matched by a maximum of R I evidence. After 2, the amount of Class P evidence falls away until about 5000 B.P. This is the period of the long regression between F and G, and the slight P-peak, 3, (which corresponds approximately to the maximum of R I evidence) may reflect dry land conditions, like the main peat beds in the long regression phases in the early Holocene. Peak 4 of Class P lies between peaks G and H of the T W curve, but it does not correspond to the R I maximum that follows G. The nearest turning point is an R I minimum, with which it corresponds to within a third of a century. Peak 5 of Class P then corresponds to within half a century with the unlabelled T W peak at 4250. Peak 6 is then about 150 years before Peak I of the T W curve. The nearest R I peak is however twice as distant. This latter falls at ca. 3500, at a minimum in the P-curve. The next P-maximum, 7, then corresponds (to well within a century) to maximum J of the T W curve, and to the minimum of the R I curve that corresponds with this. Maximum 8 of the P-curve matches neither of the other curves, but 9 coincides closely (within a half century) with both maximum K of the T W curve and its corresponding minimum of R I evidence. The P-minimum between 9 and 10 is also matched by a minimum and maximum respectively of the T W and R I curves. Exceptionally P-maximum 11 corresponds to the maximum of Class R I evidence preceding T W-peak M. It should be noted however that the R I and T W peaks concerned are separated by only a century. The final Class P peak, 12, returns to the general tendency by corresponding closely to a maximum of the T W curve, in this case peak N.

Thus out of twelve Class P maxima after 6500 B.P., only two (nos. 3 and 4) correspond to maxima of the R I curve. One other (No.8) apparently matched neither the T W nor R I curves. No.4 fell between two maxima of the T W curve, but in fact coincided with a minimum of the R I curve. In the remaining eight out of twelve cases, the Class P maxima corresponded most closely to maxima of transgression evidence.

Broadly, it seems that the smaller and the more frequent the T W peaks, the closer is the agreement of the Class P curve. This is illustrated specifically in Part III (Chapter 10) below, which deals with the period from 4000 B.P. to the present, where the data suggest the alternations of marine influence are at their most rapid rate in the Holocene.

It is notable that after 6500 B.P. many more Class P radiocarbon dates refer to thin layers of intermarine peat than before that date. It is suggested provisionally that this may reflect the higher rate of change. In the later stages of the Holocene, with only on average about a third as much time between peaks of transgressive evidence as separates the major early peaks (B, C and E), it seems reasonable to suppose that there was seldom time for a substantial vegetation cover to develop at sites vulnerable to further transgression. The graphs suggest that those peat layers which did survive were then most often those which marked the onset of the transgressions which buried them.

Dutch papers relevant to this have been quoted above. Morner's recent detailed investigations in the Kattegat area (1969) disclosed that when very thin peat layers were found between marine sediments, the lower sediment often showed a weathering crust implying a period of subaerial exposure, but the vegetation remains appeared to refer only to the immediate onset of transgression there. In the survey as a whole, the present writer found few accounts of very thin beds of peat that could be referred unambiguously to colonisation of recently abandoned marine deposits.

It would seem, then, that although regression contacts at the base of relatively substantial peat beds are plentiful (Class R), thin regression beds tend not to survive, and in general in the later Holocene thin intermarine peat beds tend to reflect transgressions.

This conclusion, like that regarding shell beds, would appear to be worth further investigation since again it appears to represent an instance where the type of approach used in the present survey might contribute to the elucidation of points that were difficult to settle in terms of the evidence available in the field at individual sites.

For the present however it is sufficient to note that because of the change in the pattern of the T/P relationship, in Parts II and III the Class P curve is no longer shown inverted. Since Parts I and II overlap between 7000 and 6000 B.P., the transitional period may be viewed in both forms of presentation by comparing Fig. 8.2 with Fig. 9.2.

(iic) In all, some 384 radiocarbon dates fall within the period to be considered in this Part. This period, however, includes the Baltic lake stages discussed in Chapter 7. The 61 Baltic radiocarbon determinations with mean values falling either prior to 9600 B.P., between 9000 and 8100 B.P., or between 7500 and 7000 B.P. are accordingly excluded here. The only exception to this is that their laboratory numbers are listed for completeness when the overall sequences at individual sites are described. However, they are distinguished by brackets in these lists and are not used in argument. The remaining 79 Baltic dates are included in the discussions which follow. 30 of these fall after 7000 B.P., and these may be regarded in the same way as dates from outside the Baltic since the Scandinavian literature and the independent study pursued in terms of the graphs in Chapter 7 seem unanimous that the Baltic remained an arm of the sea after that date. The 49 earlier dates must however be treated with circumspection because of the complications of interpretation introduced by the lake phases, even though they themselves appear to fall outwith the lake periods as defined in Chapter 7. They are underlined with a broken line in the following lists (Section iii). When these 79 Baltic dates are admitted, a total of 323 dates is available for the period between 10300 and 6000 B.P.

The overall patterns of the Class T R W I S U and P evidence are shown in Fig. 8.2. (For the reasons given in Chapter 5, the Class X and M evidence is not graphed. It is however discussed at a later stage in this Part. The same procedure is followed in Parts II and III.)

It will be seen from the diagram that the occurrence of a transgression in Phase A gains no clear support from the P U or S curves. On the interpretations outlined above, however, the maxima of the P and general S curves at ca. 9800 and 9700/9600 B.P. tend to support the R I curve in suggesting a period of regression before transgression Phase B.

As noted above, the Phase B peak of evidence of trangression is supported by a maximum of Baltic Class U evidence, and by minima in the P and S curves both inside and outside the Baltic, while the division between Phases B and C is in turn supported by maxima of Class P and S outside the Baltic. The probable occurrence of the Ancylus Lake stage within the Baltic at this time would appear to confirm that a decrease of marine influence is likely to have occurred between B and C.

The peak of transgressive evidence in Phase C is marked by a peak of shell dates indicating transgression (Baltic) and by a matching minimum of general shell dates (non-Baltic). The second peak of Baltic Class U evidence also falls at this time, and both Baltic and non-Baltic Class P curves show corresponding minima.

Phase D gains no support from Classes U or S or from Class P outside the Baltic. Despite this, the tendency of the Baltic Class P evidence to support D is sufficiently marked to be reflected in the curve summing the Baltic and non-Baltic Class P evidence. It will be recalled that the Baltic Class T curve also shows a minimum corresponding closely to the C/D boundary of the non-Baltic T-curve.

Again as noted above, the C/D to E maximum of regression evidence is paralleled by maxima in Classes P and S. The peak of Phase E transgressive evidence is then immediately preceded by a minor peak of non-Baltic Class U evidence, before the U curve rises towards a larger peak that corresponds closely to the Phase F transgression peak. It will be recalled from the previous examples of Class U peaks (discussed in the previous chapter) that these dates of wood from drowned forests tend to slightly precede the dates of corresponding transgressions. The Phase E Class T W peak is also confirmed by a (non-Baltic) maximum in shell evidence specifically indicating transgression, with a corresponding minimum in the general S curve. It is to this phase that the Class P peak discussed above corresponds.

In general, then, it may be said that the overall patterns of the different classes of evidence tend to support the greater part of the initial hypothesis in this period, but that they cast little light on the problematic

Phases A and D.

(iii) The multiple-date sites

210 of the radiocarbon dates (plus a further 37 "Lake stage" Baltic dates) come from sites for which series of dates rather than solitary radiocarbon determinations are available. 74 of the 112 sites involved are located on the seaboard outside the Baltic, and 38 are inside the Baltic. These sites are listed below. So that nearby sites appear together, the entries are ordered geographically, and to facilitate cross reference with the maps which follow, they have been classified in 1^{o} by 1^{o} blocks of latitude and longitude. Unless the suffix "W" has been added, all longitudes may be assumed to be eastings.

For brevity, each site is identified by a serial number (underlined, preceding the laboratory numbers). These serials were again allotted in geographical order, and their validity extends throughout the three sections in which the Holocene is discussed here (e.g the serial "166", figuring in the lists of Parts I, II and III, refers in each case to the same site at Waarde in the Netherlands, and so on.)

Where the final laboratory number quoted here for a site is underlined with a solid line, this indicates that while that determination is next in the series at that site, it falls after 6000 B.P. Although these dates are given here in the interests of continuity, they are omitted from the totals given above.

For conciseness, the terms "multiple-date" and "single-date" will be used in the text that follows to distinguish between those sites for which two or more radiocarbon determinations are available and those represented by only one date in the Holocene. The former will be given more weight in discussion, for two reasons. Firstly, because data from multiple-date sites are considered more reliable, in that the dates at these sites are susceptible to the check for inversions of date order against stratigraphic order, and secondly because even where undated strata occur between those from which radiocarbon samples were taken, the dates set limits for the intervening changes.

The latter advantage over the single-date sites is valuable, because without control of this nature attempts to extrapolate the dates of undated stratigraphic changes must generally be considered unreliable. For example, in the data considered here, except in the most general terms bed thicknesses can not be regarded as a guide to the length of time that a stratum has taken to accumulate. At an early stage in the present study it became clear that varying conditions of sedimentation, preservation and compaction meant that even in superficially similar environments of deposition the relationship between bed thickness and time varied markedly, not only from site to site but within the sequence at individual sites. It therefore seemed preferable to avoid such speculations, and to place most reliance on a comparison of the number of changes of marine influence between known dates at multi-date sites.

In the section which follows, because of their exceptional circumstances, Baltic sites are first listed and discussed separately before being included with the sites from the remainder of the western seaboard of Europe.

<u>(iv) Baltic multiple-date sites: before 6000 B.P.</u>

66° 24°	<u>7</u>	(Su 36), Su 31.
61° 17°	<u>8</u>	(St 750), <u>St 712</u>.
	<u>9</u>	(St 779), St 782, St 715.
	<u>10</u>	St 754, St 753.
	<u>11</u>	St 749, St 710.
	<u>12</u>	St 774, St 719, <u>St 781</u>.
61° 16°	<u>21</u>	(St 875), St 879.

60°	28°	22	Su 16, Su 11.
60°	27°	23	(Su 43), Su 37, Su 65.
60°	25°	24	Su 13, (Su 61), Su 60, (Su 53), Su 41, Su 62, Su 59, Su 57
59°	16°	37	(U 2022), U 648, U 2055.
59°	14°	38	(U 269), (U 218).
58°	24°	45	TA 122, TA 123.
58°	16°	47	(U 2030), (U 2029), (U 528), (U 529), U 566, U 565.
		48	(U 43), U 41.
57°	18°	55	(U 428), U 2071.
		56	St 1634, St 1638, St 1641, (St 1635)
		57	St 1611, St 174.
		58	(St 1556), (St 1553).
		59	(St 1563), (St 1564).
		60	St 1560, St 1628.
		61	St 1616, (St 1610), (St 1613), (St 1618), (St 1609), St 1612, St 1617.
		62	St 1590, (St 1588), St 1589.
		63	St 1583, St 1582, St 1584.
		64	(St 1621), (St 1622), St 1620, St 1619.
56°	16°	77	U 1016, U 488, U 489, U 485, U 571.
		78	St 1068, St 1091.
		79	(U 486), U 514, U 495.
55°	13°	98	St 1194, St 1196, (St 1195)
55°	12°	99	St 1233, St 1215, St 1216.
		100	St 732, St 720.
55°	10°	101	(K 594), (K 595).
		102	(K 942), K 926, K 922.
		103	(K 592), (K 593)
		104	K 852, K 1028, K 857.

54° 10° 106 (KI 208), KI 207.

107 (KI 210),(KI 209).

108 (KI 214), (KI 215).

Among the 38 Baltic sites, 9 were represented only by dates falling in potential Lake phases (as defined in Chapter 7). These were sites 8, 38, 55, 58, 59, 101, 103, 107, and 108. Two others (22, 37) were represented by only one date that was free of the Lake stages, prior to 6000 B.P., but by other dates after 6000 B.P. They are therefore considered along with the evidence for the period between 7000 and 3000 B.P. Sites 77 and 99 are disregarded because of a definite (77),and a possible (99) inversion of date order with respect to stratigraphy (these sites were discussed in Chapter 5, where 77 was identified as Mellosa and 99 as Falsterbo). Five sites were of limited value, because they each yielded only one date not coinciding with Lake stages. These were sites 7, 21, 48, 98 and 106. Of these, site 98 must be discarded because pollen analyses suggest strongly that the determination of St 1194 is in error. Except for a slight disagreement involving one date from site 21 (St 879), the evidence from these sites appears to be consistent with the initial hypothesis.

22 Baltic sites remain to supply more specific information on how far phases identified on the graphs represent single and distinct events. Because of the potential Lake stages defined above, their evidence will not be considered in relation to Phase A, or the regression period B/C, or the part of Phase D after the maximum at 7400.

The possibility that individual peaks on the graphs may embrace several stratigraphically distinct events will be considered first.

13 dates at 6 of these sites fall within Phase B. Sites 56 and 104 have 3 such dates each, sites 45, 57, 78 and 102 have 2 each. At each site, these sets of dates refer to peat or other vegetation samples which are not

separated by marine sediments, and in none of these cases is there any evidence of multiple transgressions within the period described as Phase B.

11 dates at 8 sites fall within Phase C: sites 47, 60 and 100 have two dates each, and 23, 24, 61, 62 and 63, one each. In the same way there is no indication at any of these sites of multiple transgressions within Phase C.

15 dates at 11 sites fall within Phase E. Sites 9, 10, 11, 64, 77, have two dates each; sites 12, 23, 61, 62, 63, 79 have one each. Again, there is no indication at any of these sites of multiple transgressions within this Phase.

It would thus seem that the Baltic evidence offers no contradiction to the implication of the graphs that Phases B, C and E each represent single events.

The occurrence of the potential Lake stages limits the value of the Baltic evidence for assessing how far the Phases generally identifiable on the graphs represent stratigraphically distinct events. In no case does the stratigraphy of a Baltic site appear to exclude the possibility that all the Phases falling before 6000 B.P. represent distinct events. However, at 13 of the 22 sites no positive evidence of their distinctness is available (generally because dates outside potential Lake stages refer to the period of only one Phase at these sites). The stratigraphy at sites 23, 24, 61, 62 and 63 suggests however that Phases C and E were separate, and this would appear to be confirmed by sites 9, 10 and 11. These each show two stages in the isolation of lakes from the sea, which appear to confirm that regressions D/E and E/F were separate. (9 St 782, 10 St 754 and 11 St 749 coincide with D/E, and 9 St 715, 10 St 753 and 11 St 710 with E/F)

In so far as the Baltic's intermittent contact with the oceans prior to 6000 B.P. allows an assessment, it would thus seem that its evidence is not inconsistent with the initial hypothesis.

(v) Multiple-date sites outside the Baltic: before 6000 B.P.

69°	16°	3	T 294, T 295, T 270.
		4	T 381, T 380, T 353.
		5	T 272, T 296.
59°	10°	40	T 241B, T 241A.
		41	T 117, T 121.
		42	T 119C, T 119A, T 119B.
58°	12°	49	Lu 219, Lu 220.
58°	11°	50	Lu 158, Lu 157.
		51	Lu 218, Lu 217, Lu 216.
		52	Lu 139, Lu 140, Lu 141, Lu 137, Lu 136
58°	7°	53	T 81, T 80.
		54	T 209, T 281.
57°	12°	67	St 1894, St 1892, St 1895, St 1893, St 1890.
		68	St 1531, St 1533, St 1532.
		69	St 1529, St 1530, St 1528.
		70	St 1292, St 1291.
		71	St 1498, St 1492, St 1500.
		72	St 1496, St 1505.
		73	St 1511, St 1510.
57°	11°	74	Lu 21, Lu 22, Lu 20.
56°	12°	82	St 1707, St 1497.
		83	St 1813, St 1817.
		84	St 1816, St 1814.
		85	St 2172, St 1820.
		86	St 1711, St 2214.
		87	St 1818, St 2177.

		88	St 2010, St 2012.
		89	St 2005, St 2011.
		90	St 2004, St 2170.
		91	St 2164, St 2165.
		92	St 2203, St 2176.
		93	St 2289, St 2288.
		94	St 2287, St 2345.
		95	St 2337, St 2335.
		96	St 2510, St 2527.
		97	St 2895, St 2894, St 2894 dup., St 2893.
54°	9°	109	Hv 622, Hv 629, Hv 631.
		111	Hv 184, Hv 183, Hv 181.
		112	Hv 225, Hv 222, Hv 226.
		113	Hv 218, Hv 219.
54°	5°W	120	LJ 904, LJ 903, LJ 908.
		121	Q 632, Q 632 dup., Q 770.
		122	I 1198, I 1199.
54°	2°W	123	Q 260, Q 261.
54°	1°W	124	BM 80, BM 81, BM 83, BM 90.
53°	10°	125	Hv 561a, Hv 662.
53°	8°	131	Hv 104, Hv 103, Hv 102.
		132	Hv 111, Hv 110.
53°	6°	142	GrN 621, GrN 637.
		144	GrN 606, GrN 601.
52°	4°	150	GrN 2619, GrN 1621, GrN 1622.
		151	GrN 1054, GrN 1057, GrN 1650.
		152	GrN 1618, GrN 1617, GrN 1620.

153 GrN 639, GrN 633.

154 GrN 455, GrN 1060.

155 GrN 1123, GrN 1131.

156 GrN 1633, GrN 1135.

52^{o} 4^{o}W 157 Q 380, Q 382.

51^{o} 4^{o} 166 GrN 1112, GrN 348.

167 GrN 240, GrN 228, GrN 222.

172 GrN 2283, GrN 1662.

173 GrN 201, GrN 186, GrN 203.

51^{o} 3^{o} 177 GrN 1580, GrN 421.

178 GrN 1626, GrN 1627.

51^{o} 3^{o}W 182 I 2690, I 2688, I 2689.

184 Q 275, Q 274.

185 Q 660, Q 661, Q 662, Q 663.

51^{o} 0^{o} 189 Q 790, Q 790 dup., Q 791.

50^{o} 1^{o} 192 Q 834, Q 835, Q 831.

193 Gif 396, Gif 397.

194 Gif 398, Gif 399.

195 Gif 764, Gif 763 bis.

49^{o} 0^{o} 197 Sa 69, Sa 70, Sa 71, Sa 68.

49^{o} 0^{o}W 198 Gif 728, Gif 371.

Among these 74 multiple-date sites located on the western European seaboard outside the Baltic, 23 were represented by only one date prior to 6000 B.P., (54, 70, 86, 92, 94, 113, 122, 123, 125, 132, 142, 144, 153, 154, 155 156, 157, 166, 177, 184, 193, 194, 198). These are accordingly considered primarily along with the later periods, rather than here.

Of the remaining 51 sites, 9 are disregarded here because of problems discussed in Chapter 5. (112, Husum, was published incorrectly; 85, Kattegat B 176, and 93, Sjaellands Odde, had clearly been disturbed since deposition; and 49, Vasbosjon, 69, Torp, 74, Brakemotet, 123, Silverdale, 185, Port Talbot, and 197, Mesnil, all showed some degree of inversion of date order relative to stratigraphic order.) It was also considered best to omit 88 because there seems good reason at that site to suspect an unconformity. Sites 195 and 124 are stratigraphically irrelevant, for present purposes (e.g. the latter refers to a series of dates made in the course of methodological experiments), while sites 67 and 68 are located in ancient deltas and although they yield brackish diatoms the nature of their relationship to marine changes seems sufficiently controversial (e.g. Morner 1969) to demand their exclusion here. Site 91 must also be omitted because the published interpretation is not clear. This leaves 36 sites with clear series of dates in the period 10300 to 6000 B.P. from beyond the Baltic, or 58 when the Baltic sites are included.

With the date sequences at these non-Baltic sites as a starting point, the proposed Phases will now be examined in turn. At each stage, the data from sites represented only by single dates will also be taken into account, and where relevant the Baltic data, both from the multiple dates inventoried above and from single date sites, will also be considered.

(vi) Phase A

As noted above, this Phase is not clearly defined by the graphs but it would appear to start sometime before they commence at 10300 B.P., reach a maximum about 10000 B.P., and end as evidence of regression builds up towards 9750 B.P. It thus lies within a period in which Baltic evidence is inadmissible, in that it was probably dominated by the "Baltic Ice Lake". Furthermore, as has been indicated, evidence from the western European seaboard outside the Baltic is not plentiful in this period. Only 22 non-Baltic dates have mean values falling between 10300 and 9800 B.P. and several of these have problems of interpretation. Any conclusions regarding the validity of this Phase thus rest on a much less substantial basis than in the majority of the later cases considered below, and must accordingly be viewed with caution.

Six of the dates yield little useful information in the present context. Adequate details of St 2003, St 2165 and St 2175 have yet to be published. NPL 101 from the Channel Tunnel site demonstrates no more than that there has been a minimum relative rise of sea level of 36 metres there since 9920 ± 120 B.P. The peat sample dated contains no evidence of immediate marine or estuarine influence. St 2021 and St 2038 refer to meanders in the Viskan valley on the Kattegat (57° 12°). These have been related to changes of sea level by Wenner, but doubt has recently been cast on the validity of the sampling (Morner, 1969) and it seems best to suspend judgement until Widell's work, now in progress, is completed.

None of the dates falling prior to 9750 B.P. suggest that more than one transgression is represented in the period designated Phase A. However, only seven multiple-date sites figure in this period (3, 4, 71, 73, 89, 90, 109) and each of these is only represented by one date, so this is inconclusive.

The earliest dates are from sites 73, 89 and 88. These all refer to the isolation of lakes from the sea, in the Swedish Kattegat area (73, St 1511 10400 ± 170, 89, St 2005, 10285 ± 120, and 88, St 2010, 10000 ± 330). Little may however be concluded from these. In the case of St 1511 there is a conflict with pollen evidence, and since St 2005 and St 2010 are suspected to rest on unconformity, they probably only give a minimum age for the land stages there.

From then onwards, however, the evidence becomes more informative. At Laholm (56° 12°) and off Anholt Island (56°11°), peats representing respectively lacustrine and beechwood environments were drowned by a marine transgression

(Laholm U 455, 10060 ± 140; Anholt St 2344, 9945 ± 120). This was confirmed at site 71 (57° 12°), where the diatom diagram for this period shows marine influence increasing to a maximum at 9810 ± 110 B.P. (St 1498) and then declining.

Sites in both west and south Norway also show a transgression maximum that coincides with this date. In Sogne Fjord (61° 6°), the sea fell from the marine limit of the Tapes period at about 9945 ± 465. The limit in the Oslo area (59° 10°) which corresponds to this according to the geomorphological and stratigraphic investigations of Tanner, Carlsson and Holtedahl (inter alia) did in fact yield dates which agree closely: T 178, 9895 ± 245 at Kjelleollen Tonsberg, and T 118, 9745 ± 375 at Foss Teglverk, Oslo.

At other sites, too, there is clear evidence that marine transgression reached a maximum and was then replaced by a short regression in this period. At six sites thin peat beds intervene between marine sediments. This is so at two sites at Ramsa in north Norway (69° 16°) (3 T 294, 9800 ± 200; 4 T 381, 9890 ± 240) and at three west Swedish sites: Alvaleken ($56^\circ 12^\circ$) 90 St 2170, 9780 ± 200; Hundested/Grena ($56^\circ 11^\circ$) St 2174, 9725 ± 120; B 124 (BP 1232) in the Viskan area ($57^\circ 13^\circ$) St 1710, 9700 ± 120; and also on the Danish island of Laeso ($57^\circ 11^\circ$), St 2753, 9700 ± 120.

From information published so far it is not clear whether the peat bed dated Hv 622 9750 ± 115 at Delve, Site 109, on the German North Sea coast (54° 9°), rests on a marine deposit, although it was certainly later overlaid by marine clay like the beds at the six sites just listed.

In general, then, it would seem that although relevant radiocarbon dates are not plentiful prior to 9750 B.P., those which are available do appear to follow a pattern through time which is not inconsistent with the concept of a single transgression in the period designated Phase A. There seems no indication of more than one transgression at any site within this period, and the stratigraphy of at least six sites (listed in the last paragraph) suggests specifically that Phase A is distinct from Phase B.

(vii) Phase B

According to the graphs, this phase starts about 9750 B.P., with evidence of transgression increasing to a maximum at 9150 before falling off until it reaches a minimum at 8550 B.P. In contrast to Phase A, this period is marked by an abundance of positive evidence of transgression. Furthermore, according to the study in the previous chapter, it seems safe to regard Baltic evidence as reflecting the same influences as the remainder of the seaboard for some 700 years of this period, starting 9600 B.P. Altogether, almost 90 radiocarbon dates are available from this phase.

Some of these are however of limited value in the present context. Single-date sites in this category will be dealt with first. NPL 86, 8580 + 830 - 755 B.P. is surrounded by such a large area of statistical uncertainty that it seems best to disregard it. Gif 342, 8720 ± 300 refers to a mammoth tooth from the seabed in the English Channel. The organic fraction was too small to be dated so total carbonate was used, and the result seems sufficiently anomalous in paleontological terms to suggest contamination. The geological interpretation of U 2101 was admitted by the submitter to be based on an assumption, and furthermore the sample had to be diluted with ancient carbon to obtain an assay. It seems that either the assumption or the determination is in error (Radiocarbon v. IX, p.531). Q 105 and Q 181 refer to peat with neither marine nor brackish species and are valuable only for demonstrating the minimum rise of sea level since their dates at the Leman Ower Bank and Poole Harbour, respectively. K 857, 922, 926, 942 and 1028 supply similarly limited information regarding the Danish islands. Some Norwegian shell dates are also of primarily local interest since their precise relationship to transgressions or regressions is unclear. These are T 120, T 125, T 179, T 180 bis, T 231, T 286 and T 287. 14 of the Baltic dates, while not falling within the Ancylus Lake period as defined in the previous chapter, refer only to dryland conditions preceding the onset of the Ancylus transgression, and cast no direct light on Phase B. These are TA 77, 78, 122, 123, St 120, 174, 333, 1068, 1091, 1611, 1634, 1638, 1641 and U 2071.

25 multiple-date sites are represented by one or more dates in the period of Phase B. Of these, 5 are of limited value here. The reasons for excluding site 67 (located in a delta) have already been stated earlier in this chapter. Site 4 is represented by T 380, and Marthinussen (Radiocarbon VI, p.282) draws attention to serious local difficulties concerning this date, which may be interpreted in at least three different ways. It is considered advisable to disregard it here. Sites 41, 42 and 50 are all shell beds, and their interpretation is also problematic. Indeed at site 50, it can be proved that the bed is a mixed deposit, made up of shells with a 2000 year age range, and date discrepancies suggest this may also be the case at site 42.

Of the remaining 20 multiple-date sites, three sites, each represented by two dates within the period of Phase B, show stratigraphic sequences that conflict with the interpretation of Phase B as a single cycle of marine transgression peaking about 9150 B.P. These sites are 51, 53 and 95. The sequences at sites 53 and 95 seem similar in that both apparently show regressions about 9400 and increases of marine influence about 8800 B.P. The regressions are dated by 53, T 81, 9410 ± 220 and 95, St 2337, 9355 ± 185.

The evidence is not however clear. In reporting T 81, Gabrielsen noted that further investigations were necessary at that site, which is located at Hartmarksfjord in Norway (58° 7°). At site 95, at Hanhals in west Sweden (57° 12°) St 2337 marks the point where blue marine clay is succeeded by an indeterminate deposit, which may be lagoonal or may represent lacustrine conditions near to or at sea level. The ensuing increase in marine influence at these sites is dated 95 St 2335, 8860 ± 105 and 53 T 80, 8770 ± 100.

Site 51 at Kobengtserodssjon ($58^{o}11^{o}$) also registers a transgression at that time. The lake there was flooded by sea water at 8980 ± 146 (Lu 218). But after that , Site 51 shows further alternations of marine influence apparently not represented at other sites. This lake was soon afterwards once again isolated from the sea, then re-connected before being isolated once more at the end of Phase B. (Lu 217, 8590 ± 153).

At some sites at least, there are thus indications of more complex changes than the graphs suggest. The time pattern of all relevant dates falling within Phase B will now be considered, in order to establish whether these variations are local or general in occurrence. The sequence will be examined in detail because this Phase is also marked by a concentration of dates involved in inversions (Inversion Group I of Chapter 5).

It will be recalled that Phase A was taken to end with the change to evidence of regression that is apparent between 10000 and 9750 B.P. Very little definite information is available concerning the next four hundred years.

There is some indication that there may have been a brief increase in marine influence immediately following the regression terminating Phase A. For instance, at 9555 ± 70 (GrN 1054), Vitgeest ($52^{o}4^{o}$) became the first dated site within the present limits of the Netherlands to be affected by a rise of watertable due to Holocene marine influence (Jelgersma 1961, 1966). Almost simultaneously with this, at Alback (57^{o} 12^{o}) in the Kattegat, St 1496 registered a change from freshwater diatoms to marine diatoms at 9510 ± 135. No corresponding morphological feature has been identified there however, and the validity of this determination has accordingly been questioned (Morner 1969).

Certainly if any general increase of marine influence did take place after the 10000 - 9750 period of regression evidence, it must have been of very short duration, because a sample from off the Danish island of Fyn ($55^{\circ}10^{\circ}$) shows marine sedimentation being replaced by peat growth at K 852 9550 ± 140.

St 185, 9455 ± 140, Kluite, and St 1707, 9400 ± 450, Site 82, then show land or freshwater conditions but in these cases it is not altogether clear whether an actual reduction of marine influence is implied, or whether the sites had yet to be reached by Holocene marine influence.

On balance, because of the shortage of evidence between 9750 and 9400, and the doubts surrounding the interpretation of some of the available radiocarbon dates, it seems preferable to postpone any conclusion on this period until further information becomes available.

After about 9400, in contrast, the evidence immediately becomes both abundant and clear in its implications. Evidence of transgression is completely dominant. Indeed, no radiocarbon date referring to a decrease in marine influence occurs between 9400 and 8750 B.P.

The earliest definite evidence of this transgression phase is from site 84, off Laholm (56° 12°). There St 1816, 9435 ± 180, dates "pitchy soil" immediately underlying marine clay. This material is characteristic of the groundwater rise at the local onset of transgression (Morner 1969). At almost the same time, KI 207, 9430 ± 85 indicates the transgression of a peat bed at site 106, Schleimunde in the Kieler Bucht (54° 10°). Again essentially simultaneously with this, Lu 16, 9420 ± 100 refers to the drowning of a pine forest off Bornholm (55° 15°). As noted in Chapter 5, this particular sample appeared not to be in situ. Tage Nilsson considered that it had been moved in the course of trawling (Radiocarbon Xii 1968, p.39), but although no conclusions can be drawn from its depth (80m.) he accepts that the date of the drowning of the pinewood is meaningful.

Then outside the Baltic at Lunnaled (57° 12°) at site 73, fresh-water mud was replaced by marine sediment. There is some discrepancy between the pollen diagram at this site and the date of 73 St 1510, 9330 ± 200, but three other radiocarbon dates from the same general area of west Sweden agree very closely. These are from separate sites in the Viskan valley. St 1899 9330 ± 110 refers to driftwood dating a rise in the level of alluvial deposition which is related by Morner (op.cit.) to a marine transgression. St 1207, 9305 ± 110 dates a thin saltmarsh deposit immediately overlain by marine clayey mud. St 1290, 9280 ± 110 refers to "pitchy soil", and as noted above this appears to afford a particularly accurate indicator of the local onset of transgression.

Inside the Baltic, St 806, 9300 ± 130 dates the drowning of a pine forest at Langoren, between islands south of Torhamn (56° 15°). Unlike Lu 16, this material is certainly in situ, and so most probably is the similar pine forest material from the seabed between Kaseberga and Bornholm, drowned at 9275 ± 120 (55° 15°) (Ostlund HG Science v. 126, 1957, p.493; Radiocarbon Index p.148).

Four sites yield dates with means falling on 9280 B.P. St 1814, 9280 ± 300 is from site 84 on the seabed off Laholm, and is a reserve sample for St 1816 (quoted above) with which it agrees to within one standard deviation. St 2169, 9280 ± 125 also comes from a seabed boring in the Kattegat (56° 12°), and marks the top of a thin layer of dy, lying between marine sediments, where it is in contact with the overlying blue sea clay. Site 94 (56° 12°) shows a rise of waterlevel dated St 2287, 9280 ± 140. St 2004, 9280 ± 130 is from site 90, Alvalaken near Gothenburg, and is from the "pitchy soil" marking the onset of a transgression. This ended a dryland phase that had lasted there from

9780 ± 200 (St 2170), when the regression separating Phases A and B had first become apparent at that site. This Alvaleken site is now above sea level, but the stratigraphy is similar to that disclosed by a boring made into the seabed outside Copenhagen which gave a very similar date (St 2347, 9230 ± 260) for the top of peat when it is overlain by a clay that appeared to represent a brackish facies.

Simultaneously with this at site 111 on the German North Sea coast at Meldorf (54° 9°) forest-bog peat was overlain by silty marine clay (Hv 184 9230 ± 100.

Two separate sites in the Viskan area of west Sweden then yield the same date. St 1698, 9155 ± 120 refers to a thin peat marking the transgression of a depression in a buried land surface of weathered blue clay, while St 1497, 9155 ± 110, site 82, refers to the top of a thin intermarine layer of peaty detritus where it passes into sea clay. Site 82 is above sea level, but site 83 is again on the sea bed of the Kattegat. A boring there (56° 12°) yielded two dates. St 1813, 9230 ± 500 referred to a land stage immediately before the development of "pitchy soil" dated St 1817, 9130 ± 120, which marks the onset of the transgression which sealed the deposit with blue clay.

All the available radiocarbon dated evidence thus points to a definite rise of sea level between 9400 and 9100 B.P.

Between 9100 and 8750 B.P., although the evidence continues to consist of transgression contacts, it is less plentiful. Indeed it comes from only three sites, and each of these has already been noted for its seemingly exceptional stratigraphy (sites 51, 53 and 95).

Sites 53 and 95, Hartmarksfjord (58° 7°) and Hanhals (57° 12°) both registered regressions just prior to the main series of Phase B transgression dates (T 81, St 2337) but then apparently maintained freshwater, or, in the case of site 95, perhaps shallow lagoonal, conditions until after 9000 B.P. Then at site 95 in 8860 ± 105 (St 2335) definitely marine sedimentation set in, to be followed at 8770 ± 100 (T 80) by a transition from peat growth to marine clay at site 53.

No information from before 9000 B.P. is available for site 51 Lake Kolbengtserodssjon ($58^{\circ}11^{\circ}$) but at 8980 ± 146 (Lu 218) a change from lacustrine to marine sedimentation was apparent there, and with this the site enters a complex period of intermittent contacts with the sea. It was isolated and then reconnected to the sea before being re-isolated at 8590 ± 153 (Lu 217, see below).

Except for these minor and as yet undated fluctuations at site 51, the last indication of increasing marine influence before about 8300 B.P. is at 8700 ± 110 (GrN 1618) when site 152 at Nieuwe Wetering in the Netherlands ($52^{\circ}4^{\circ}$) registered a rise of watertable, apparently due to marine influence reaching the area.

From 8700, however, evidence of regression builds up. For example, at Solum in west Norway (59° 9°) Lake Tjonna was isolated from the sea at 8700 ± 280.

Between about 8700 and 8300 B.P., no evidence of transgression is apparent and all positive evidence refers to regression. This is so even at Lake Kolbengtserodssjon, (Gosta Persson, Radiocarbon XIii p.435). After its re-isolation in 8590 ± 153 (Lu 217, above), Lu 140, 8400 ± 105 confirmed that truly lacustrine conditions prevailed continuously until 8330 ± 115 (Lu 141).

Site 5 in north Norway (69° 16°) showed a stage of land vegetation at 8600 ± 150, T 272. At site 125, Lauenburg, under North Sea influence in Germany, peat started to grow at 8420 ± 80, Hv 561a (53° 10°), while at Husum (54° 9°), Hv 225, 8400 ± 100 marked a land stage some time before the deposition of the "Eesch" bed (see below: the onset of this coincides with Phase C.) Morner (1969) gives a similar interpretation for a date from a peat bed at site 88 in west Sweden, 8595 ± 100, St 2012. Off Skalderviken in the Kattegat (56°12°) the base of a submerged and buried shell bed gave a date of 8430 ± 130, St 2206, and Morner concluded on several grounds that this initial stage in the accumulation of the bank should be referred to a regression, between two transgressions at that site.

In general, then, it would appear that at least parts of the sequences at Hanhals (95), Hartmarksfjord (53), and Kolbengtserodssjon (51) reflect local rather than widespread factors. The great majority of the radiocarbon-dated sites would seem to be compatible with the following sequence: -

(a) 9750 - 9400 B.P.: little evidence of major rises or falls in relative sea level.

(b) 9400 - 9100 B.P.: evidence of marine transgression is clear at many sites, and completely dominant over evidence of regression.

(c) 9100 - 8700 B.P.: evidence of transgression is restricted to a few sites, but indications of regression are still notably absent.

(d) 8700 - ca. 8300 B.P.: the available positive evidence indicates regression, and evidence of transgression is in turn absent.

Dates from sequences inverted relative to their stratigraphic order have been omitted from the examination which led to the above conclusions. It will be recalled that the cluster of inversions described as Inversion Group I (Chapter 5) falls within this period. The majority of the dates involved in this are concentrated between 9000 and 8700 B.P. As noted above, these inversions may represent a perturbation of the radiocarbon input curve, though dendrochronological data are not as yet available to check this. However, even if this is so, they fall within stage (c) listed above, and such a perturbation would thus be likely to affect dates at relatively few sites, most of which have in fact already been judged anomalous on other grounds. There seems little reason to doubt the validity of either the main transgression stage, (b), or the ensuing regression stage, (d).

It would thus seem that the use of the term Phase B to cover the period 9750 to 8550 B.P. is justified, in that with the few exceptions indicated above the evidence in this period appears to follow a unified pattern.

Reference has already been made to evidence suggesting that Phase B was distinct from Phase A. Phase B may be considered distinct from Phase C on several grounds. Most important of these is the contrast between the abundance of dates referring to marine transgression before ca. 9100 and after ca. 8300, and the dearth of such evidence in the intervening period. As already noted, from 8700 to 8300 B.P., positive evidence of regression is available, and the stratigraphy at the Norwegian, Swedish and German sites mentioned above indicates a temporary fall in relative sea level at this period. The importance of this intermission would appear to be confirmed by the whole body of Baltic evidence. As has been shown in the previous chapter, this suggests strongly that the Baltic was isolated as the "Ancylus Lake" by a decrease in marine influence at this period.

The implication of the graphs that "Phase B" was a distinct cycle of transgression would therefore appear to be compatible with the pattern of the evidence as a whole.

(viii) Phases C and D

A substantial accumulation of positive evidence of transgression is apparent between 8550 and 6800 B.P. As was indicated above, however, it is not clear from the graphs whether this should be regarded as a single phase of transgression, or whether it should be subdivided.

It will be recalled that after the main peak of Class T and W evidence, apparent both outside and inside the Baltic at 7800 B.P. and labelled C, there is a further slight peak at 7400 B.P., labelled D. The intervening minimum in the transgressive evidence at 7500 is not however marked by any Class R or I evidence from outside the Baltic. Within the Baltic, a peak of evidence of regression does follow immediately after 7800, and a minimum in Class T evidence is also represented there at 7500 B.P. However, from 7400 onwards for about 300 years, as noted in the previous chapter, the evidence from within the Baltic diverges from that on the open coastline of western Europe.

The problem of the separation of these two possible Phases is exacerbated by the fact that the second major cluster of inversions of date order relative to stratigraphic order (Inversion Group II) falls across the period that the graphs suggest as the boundary between C and D. Thus although decisive dendrochronological data are again unavailable, this period may coincide with a perturbation of the radiocarbon input curve.

Eight out of ten of the determinations involved in Group II of the inversions of date order against stratigraphic order (Chapter 5) fall within the period from 7750 to 7545 B.P.

The two exceptions which fall later, belong to site 197, Mesnil. It will be recalled that all determinations from that site were characterised by unusually large statistical uncertainties. The two aberrant Mesnil dates in fact fall within, respectively, less than one, and less than one-and-a-half, standard deviations of the range of the Group.

If this Group does in fact reflect a perturbation of radiocarbon input, it therefore seems reasonable to conclude that its effects are essentially limited to a period of about 300 years, starting just after 7800 B.P. There would therefore seem little reason to doubt the validity of the build up of transgression evidence apparent between ca 8350 and 7800 B.P., or that after ca. 7400 B.P. These stages will be documented, then the problematic period intervening will be examined.

First, however, it is necessary to consider whether any further subdivision of the proposed Phases C and D is necessary. Without prejudice to the later investigation into the distinctness of these units, for the purposes of this initial examination of the data the term "Phase C" will be used to indicate the period from 8550 to 7500 B.P., and "Phase D" the period from 7500 until 6800 B.P.

When Baltic evidence prior to 8100 B.P. and between 7500 and 7000 B.P. is omitted, some 109 radiocarbon dates are available for the period between 8550 and 6800 B.P. Of these, 64 fall before 7500 B.P. and 45 after that date.

Of the 64 dates falling within the period designated Phase C, some are of limited value.

This is so in the following 5 cases, from sites at which only one radiocarbon determination has been made. St 2167 has yet to be published adequately. The submitters of St 379, St 1208 and T 182 regard the samples as geologically unreliable because of suggestions of contamination or unconformities. TA 54 refers to a land stage which postdated the "Ancylus Lake" but by an unknown period. T 290 dates a fossil coral reef in Oslofjord but this date is also of only local interest, since the relation of the deposit to transgression or regression is not clear.

Excluding inversion sites, 27 multiple-date sites are represented in this Phase by one or more dates, and some of these are also uninformative in the present context. Sites 195 and 198 are represented only by dates that show merely that the sea had apparently not reached these sites at the time of Gif 763 bis and Gif 728 respectively.

The remaining 25 multiple-date sites are represented by 31 dates within Phase C, sites 100, 98, 77, 60, 47 and 40 being represented by two dates each. Each of these six pairs of dates refers either to samples from within the same stratigraphic units (100, 77, 60, 47) or to dates defining a single stratigraphic boundary (98, 40). None of these cases indicate more than one transgression during Phase C. Nor can any proof of a multiple transgression in this Phase be adduced in terms of the stratigraphy of those other sites represented by sequences fixed by one date within Phase C and other earlier or later dates (sites 173, 172, 155, 151, 121, 120, 113, 111, 108, 97, 89, 71, 70, 63, 62, 61, 51, 24, 23). As far as may be judged, the same is true at the 20 single-date sites represented within this period (GrN 792, 2177, 2180, 2274; K 895; KI 211; Lu 19; Q 214; St 462, 1206, 1463, 1464, 2006; T 208, 273, 293; U 466; Y 2427).

Of the 45 dates falling between 7500 and 6800 B.P., in the period proposed as Phase D, some 34 are again of limited value for the present purpose. Amongst single-date sites, this is so of T 291 which refers to shells with an unknown relationship to local transgressions or regressions. Of the multiple-date sites, St 1195, site 98, seems best disregarded because of a major disagreement with the pollen zonation. At site 42, contamination is presumed by Holtdahl in the case of T 119B, on the basis of a two-millennium disagreement with T 119A and T 119C from the same context (Radiocarbon II, p.87). St 2203 at site 92 is a shell date with the same limitations as T 291, above. Site 72 is represented by St 1505 but both pollen and diatom evidence suggests that this is unreliable because of redeposition of older material.

The 23 remaining multiple-date sites are represented by 31 dates within this period. Three of these come from site 182, and two each from sites 189, 97, 87, 64, 53, and 24. The three dates at site 182 appear to show a single rise in relative sea level, and of the pairs of dates those from sites 97 and 189 are duplicate determinations from the same samples, those from sites 64, 52 and 24 refer in each case to different parts of the same stratigraphic units, while the dates from site 87 define the change to regression that is taken to mark the end of Phase D. Thus in none of these cases is there a suggestion of more than one transgression in this Phase. The same appears to be true of the stratigraphy of the sites represented by one date within Phase D and by other earlier or later dates (sites 193, 177, 173, 172, 150, 122, 121, 108, 79, 71, 54, 48, 22, 21, 9, 3). The evidence of the eight single-date sites falling within this period also appears to support this (Gsy 135; Hv 217, 628; I 3430; St 191, 2007; T 98, 107).

It would thus seem that when considered individually neither of the proposed Phases C and D offers grounds for any further subdivision.

The evidence between 8550 and 6800 B.P. will now be set out, with the exception of that falling in the problematic period between about 7750 and 7450 B.P. As noted above, this period will be discussed separately.

After the regression separating Phases B and C, the first signs of transgression appear between 8400 and 8300 B.P. From then until 7750 B.P., evidence of transgression is dominant.

At site 67 (Rya) ($57^{\circ}12^{\circ}$), there is a transition to marine diatoms at 8365 ± 260 (St 1892). Lake Kolbengtserodssjon (58° 11°) was reconnected to the sea at 8330 ± 115 (Lu 141), according to a sample from site 52 there, although site 51 nearby suggests it may have remained isolated until after Lu 216, 8020 ± 100. Certainly at 8300 ± 250 (T 273) peat growing in a rock basin at Oftenes (58° 7°) in west Norway was buried by marine sediments.

Further north, a similar event occurred at Borgrund ($62^{\circ}6^{\circ}$) at

8250 ± 250 (T 293), and southwards, in the Netherlands, sites at IJmuiden ($52^{o}4^{o}$) and Rotterdam ($51^{o}4^{o}$) were affected by watertables rising under marine influence (GrN 2274, 8170 ± 100; GrN 2177, 8130 ± 70, respectively).

In the Eckernforder Bucht, site 107, in the south Baltic (54^{o} 10^{o}) a peat bed was transgressed simultaneously (KI 209 8150 ± 80), as were others at separate sites in the Kattegat (56^{o} 12^{o}: St 379, 8145 ± 140; St 2006, 8120 ± 210) and at Ballyhalbert in Northern Ireland (53^{o} 5^{o}W: Q 214, 8120 ± 135).

At the same time a pine wood off Landskrona in western Scania (55^{o} 12^{o}) was drowned (Lu 19, 8100 ± 100), as was a peat growth at Vendyssel in Jutland (57^{o} 10^{o}: St 2895, 8075 ± 125), and apparently an oak wood at site 98 at Sjolund (55^{o} 13^{o} St 1196, 8075 ± 100). A willow wood was also drowned shortly afterwards at site 100 off Limhamn ($55^{o}12^{o}$: St 732, 7990 ± 160; St 720, 7895, ± 115), while peat was covered by marine mud at site 69 (57^{o} 12^{o}: St 1528, 7985 ± 155).

Although the evidence is less clear, a peatbed at site 195, Fort Mahon on the French coast of the Channel (50^{o} 1^{o}) would appear also to have been influenced by the sea at this time (Gif 763 bis, 7980 ± 190). Certainly Sassenheim on the North Sea coast (52^{o} 4^{o}) was affected by a watertable rising under marine influence at 7970 ± 60 (GrN 792), while at the same time shells attributed to a transgressive phase occur at Tvaersted A in Jutland (57^{o} 10^{o}: K 895, 7975 ± 165). At Vaage (57^{o} 7^{o}), on the north site of the Skagerrak, a basin was invaded by the sea at 7950 ± 150 (T 208).

In the Baltic, the post-Ancylus marine conditions reached a first maximum at Porvoo (60^{o} 25^{o}, site 24) about 7950 ± 190 (Su 60), while further up the Gulf of Finland at Virolahti (site 23, 60^{o} 27^{o}), definite marine diatoms appeared at 7835 ± 170 (Su 37). At this time, on the west coast of the Baltic at Vajern (site 47, 58^{o} 16^{o}) shells dated U 566, 7830 ± 120 and U 565 7810 ± 120 correspond to a transgression phase (Radiocarbon VII, p.462), while on Gotland Island (57° 10°) peats were transgressed at Gurpe (St 1560, 7835 ± 95; St 1628, 7815 ± 110) and Dynisse (St 1616, 7815 ± 90).

Meanwhile, in the North Sea, sites at Rhoon (51° 4°, GrN 2180, 7940 ± 75) and Bouwlust (site 172, 51° 4°, GrN 2283, 7850 ± 100) were affected by watertables rising under marine influence, while at Meldorf (site 111, 54° 9°, Hv 183, 7825 ± 100) forest bog peat was overlain by silty marine clay. This site incidentally shows the distinction between the transgression of Phase B and that of Phase C particularly clearly. These are represented in that area by strata known locally as the Barlt and Eesch beds (Muller 1962) and at site 111 dates on a single bore confirm unambiguously that they there drowned peatbeds at 9230 and 7825 B.P. respectively.

A peatbed was also transgressed at a site in the KeilerBucht in the south Baltic (54° 10°) at this time (KI 211, 7810 ± 120), as were others at 3 different sites in the Viskan area of west Sweden (57° 12°): St 1464, 7800 ± 330; St 1280, 7795 ± 90; St 1208, 7795 ± 150. The pollen count for the last of these, and for a marine clayey mud dated at site 98 ($55^\circ 13^\circ$, St 1194 bis, 7795 ± 100) seemed somewhat anomalous however.

Some doubt also surrounds the interpretation of Y 2427, 7790 ± 160, from the submerged and buried forest off the Cumberland coast (54° 3°W). The date would seem from the phrasing of the report (Andrews, Radiocarbon XIii, p.588) to represent the time when the forest bed was submerged at that site, but this is not altogether clearly stated.

However, GrN 1057, 7780 ± 75, site 151 at Uitgeest (52° 4°) represents a clear transgression contact, as do the samples in contact with "pitchy soil" at separate sites in the Viskan area (57° 12°), St 1463, 7775 ± 100, and St 1206, 7750 ± 110.

At this time in the Baltic, two further sites on Gotland Island (57° 18°) show peat transgressed by the sea (site 62, Mallingsmyr, St 1590, 7770 ± 190; site 63, Snoder, St 1583, 7750 ± 90). What is apparently a transgression contact is represented simultaneously at Tomaselv in Arctic Norway (70° 29°). The top of a thin peat layer, located just inside the crest of a shore bar is overlain by a layer of shore gravel, and gives a date of T 182, 7750 ± 150. On morphological grounds, Marthinussen had expected this to correspond to his Tapes I line, which he dates at 6500 B.P. (cf. Phase E, below). In retrospect he therefore speculates that a top layer of peat may have been removed by marine abrasion, but it is not clear whether this or his original interpretation is correct.

Even if all the dates about which doubt has been expressed are rejected, the data listed above remain an impressive concentration of evidence of marine transgression. Furthermore, in the 600 years between ca. 8350 and 7750, it is matched by a notable shortage of any evidence of transgression.

The small peak of Baltic evidence of regression following immediately after the maximum of the transgression curve at 7800 has already been noted, in the last chapter. The dearth of Class R and I evidence outside the Baltic during this period has also been discussed previously. Indeed, there only two dates (both from site 40) suggest a decrease in marine influence during this period, and even this is by no means certain, for at Fossantjern, (59° 10°), although a lake was apparently isolated from the sea between T 241B, 8000 ± 300 and T 241A, 7900 ± 250, the pollen count suggests a somewhat later date. It may be significant that the large statistical errors of the radiocarbon determinations place both these dates within one standard deviation of the upper limit of the dates forming Inversion Group II.

The dates falling between 7500 and 6800 B.P. will now be considered.

The majority of these also follow a clear pattern, with transgression dominant for the first 500 years, and then regression for the remainder of the period. As will be shown below, although a limited amount of evidence presents problems of interpretation, the division at 7000 B.P. is notably well-defined in terms of dates referring to definite increases or decreases of marine influence. Dates of definite increases of marine influences will be dealt with first.

Between 7500 ± 150 (Q 632i) and 7345 ± 150 (Q 632ii) brackish lagoon clay was deposited at Ringneill in Northern Ireland (54° 5°W), and simultaneously marine gyttja began to be deposited in Lyngdal (61° 6°) in west Norway (T 107, 7475 ± 215). In Arctic Norway at site 3, Ramsa ($69^{\circ}16^{\circ}$), the top of a peat bed is dated T 270, 7400 ± 150 where it is overlain by shore gravel. Marthinussen had again expected this gravel to belong to his Tapes I line (6500 B.P.), and as in the case of T 182 at Tomaselv (70° 29°), quoted above, he speculates on the possibility of erosion of the top of the peat bed. However, at Ravenmyr in the Sognefjord area (site 54, 58° 7°) a transgression contact occurred at exactly the same date, 7400 ± 250 (T 209), as did another on the Swedish coast of the Kattegat ($57^{\circ}12^{\circ}$) at Poppelmannagaten, St 2007, 7400 ± 100.

At Vendyssel in north Denmark (site 97, $57^{\circ}10^{\circ}$) a stage with marine/brackish diatom flora provided two dates, both listed as St 2894, 7400 ± 120 (carbonate included), and 7065 ± 135 (carbonate removed). Both these determinations fall within this period of increased marine influence.

St 1621, 7380 ± 90 dates transgressed peat at site 64 (57° 18°), Helgmyr on Gotland Island, while at 7370 ± 100 (GrN 1662), peat growth was succeeded by sea clay deposition at Bouwlust (site 172, 51° 4°) in the Netherlands.

Off the Baltic coast of Germany at Fehmarn (site 108, 54° 10°) a peat bed was transgressed at 7300 ± 100 (KI 215), while at 7240 ± 210 (GrN 186) at Brandwijk in the Netherlands (site 173 $51^{\circ}4^{\circ}$) the effects of a watertable rising under marine influence became apparent. At 7220, transgression contacts were registered by "pitchy soil" at site 87 (56° 12°) at Ekilstorp in south-west Sweden (St 1818, 7220 ± 150) and by the submergence of the top of a layer of organic detritus at site 122 (54° 5°W) at Woodgrange in Northern Ireland, (I 1198, 7220 ± 175). At the same time, GrN 1580, 7210 ± 90 registered a watertable rising under marine influence at Veere (51° 3° site 177) at the south end of the North Sea.

At 7100 ± 170, Su 63 registered the beginning of the maximum of Hyyppa's "Litorina II" transgression at Porvoo in south Finland (60° 25°, site 24).

At 7070 ± 100 (GrN 2619) site 150 at Alphen (52° 4°) in the Netherlands was affected by a watertable rising under marine influence, while at 7010 ± 130, I 3430 marked the onset of brackish conditions at the Oxford in Suffolk (52° 1°).

On Gotland Island at site 64, Helgmyr (57° 18°), transgressed peat yielded a date of 7005 ± 140 (St 1622), while at site 22, Koivisto (U.S.S.R.) on the Karelian Isthmus (60° 28°), peat under a sand bar apparently marked the beginning of a marine transgression at that site. In this last case however it is not clear where the sample was placed in the peat bed, and the date (Su 16, 7000 ± 180) may merely represent part of a land stage preceding a later transgression. This site is involved in the confusion surrounding the use of the terms "Litorina I" and "II", noted in the previous chapter. Hyyppa attributes it to "Litorina I" although at other sites in the Gulf of Finland (e.g. Porvoo, quoted above) he also applied this name to features which have since yielded radiocarbon dates falling between 8200 and 7950 B.P. The literature therefore offers no clear guide to the interpretation of this date.

As suggested above, with very few exceptions (listed below) the dates referring to definite increases and decreases of marine influence have mutually exclusive distributions in time, with the dividing line about 7000 B.P.

The dates showing local regressions will now be listed.

In the Harmanger area of Baltic Sweden (61° 17°), site 9 was isolated from the sea between St 779, 7070 ± 200, and St 782, 6940 ± 160, while at Alsmyren, site 21, (61° 16°) St 879, 6990 ± 80 also marked the start of the accumulation of freshwater peat on the isolation of the site from marine influence. (St 875, 7360 ± 100 dated the latter part of the marine stage at that site. Simultaneously, further south at site 48, Bygdslatten ($58^{\circ}16^{\circ}$) the sea began to fall away from a Litorina limit between U 43, 7110 ± 130 and U 41, 6960 ± 130.

Meanwhile outside the Baltic, at site 71 in the Viskan valley off the Kattegat (57° 12°), St 1500, 6970 ± 90 marked the limit between the falling curve representing marine diatoms and the rising curve of freshwater diatoms. Further south on the west Swedish coast at site 87 (56° 12°) freshwater peat growth succeeded marine sedimentation at 6855 ± 90 (St 2177).

As noted above, during Phase B, the sequence of Lake Kolbengtserodssjon (58° 11°) appears to have been atypical. At this time however it conforms to the general pattern. At site 52 there, it shows a transition from marine to lacustrine deposition at 6870 ± 120 (Lu 137) and this is confirmed by Lu 136, 6810 ± 120 which dates lacustrine material from 10 cm above the regression contact.

At this time, Su 62, 6810 ± 165 immediately precedes the retreat of the sea from the site 24 at Porvoo in Finland (60° 25°). The period under immediate discussion ends at 6800 B.P., but it may be noted that as the graphs suggest evidence of regression in fact continues after that date (e.g. St 754, 6780 ± 155, St 749, 6750 ± 140, Q 401, 6681 ± 130).

The remaining radiocarbon dates from between 7500 and 6800 fall into two classes, those which do not lend themselves to a precise interpretation in terms of transgression or regression, and those which appear to conflict directly with the general pattern demonstrated above.

Only three dates appear to fall into the latter class. One of these conflicts with the generalisation that in this phase evidence of transgression does not occur after about 7000 B.P. This is Su 41, 6870 ± 165 from site 24, Porvoo, which Hyppa takes to indicate the "beginning of the Litorina II stage". This determination appears to conflict with several others from the same site, notably with Su 63 which put the "start of the maximum of Litorina II" at 7100, and Su 62, mentioned immediately above. It thus does not appear to weaken the validity of the generalisation to any material extent.

The remaining two dates may suggest decreases in marine influence in the period before 7000 B.P., which otherwise appears to be totally dominated by evidence of transgression. Again, however, the apparently exceptional evidence is by no means clear in its implications.

T 98, 7200 ± 270 from Smestad by Oslo refers to wood from a humus rich layer limited by sand above and by clay beneath. This would appear to represent a local regression phase. Because of the large area of statistical uncertainty surrounding this determination, it would be difficult to establish that this was in fact different from the main group of regression dates listed above (T 98 falls within less than one standard deviation of 7000 B.P.). Furthermore, in reporting the date (Radiocarbon I, 79) Jul Lag and Tollef Rugen noted that the sample had required special pretreatment since it appeared to have been contaminated, so it would seem unwise to place much weight on this date.

The other suggestion of regression in this period is from the Harmanger area, where site 8 was isolated from the sea at 7160 ± 140. Again, even the one standard deviation limit for this determination falls very close to 7000 B.P. (within 20 years of that date).

Thus in none of these three cases does there seem to be grounds for doubting the overall validity of the generalisation that transgression was replaced by regression about 7000 B.P. Indeed, it is not even clear that any of the sites were individually aberrant.

The remaining dates all refer to peat beds buried in marine deposits, but in these cases either the locations of the samples in the bed or shortcomings in publication do not permit clear individual conclusions on the precise geological implications of the dates.

When these dates are considered as a whole, however, it is notable that they form a definite grouping in time. This group coincides with the phase of general regression defined above, and in general appears to confirm it. Except for I 2690, 7360 ± 140 (which is reported in ambiguous terms and may in fact represent a transgression contact), the remaining nine dates all fall after 7000 B.P. or within one standard deviation of that date (193, Gif 396, 7150 ± 300; 189, Q 790, 7120 ± 120; Hv 217, 7100 ± 125; 182, I 2688, 7060 ± 160; St 191, 6975 ± 110; Gsy 135, 6950 ± 170; Hv 6945 ± 90; 189, Q 790ii, 6940 ± 120; 182, I 2689, 6890 ± 120).

In general, then, in the period between 8550 and 6800 B.P., it seems safe to conclude that transgression completely dominates the evidence from 8350 to 7750, and from 7500 to 7000 B.P., with regression prevailing completely from then onwards. Only the problematic period between 7750 and 7500 B.P. remains to be considered, along with the bearing of this period on the distinctness of the proposed Phases C and D.

As noted earlier, eight out of ten of the dates involved in Inversion Group II fall between 7750 and 7545 B.P. Seven of these come from sites 68, 77 and 99. The eighth date falling within this period, and the remaining two dates lying outside it, are from site 197, Mesnil. This site will be disregarded here because the area of statistical uncertainty surrounding each of its dates (± 350) is at least twice as large as that of most other dates in the

Group, and it is accordingly considered that its incorporation would not add to the reliability of the present investigation.

On the same grounds, although the stratigraphy at the transgression contact and isolation contact dated respectively by Lj 904, 7650 (site 120) and U 466, 7500 has been reliably defined (with the support of diatom analyses in each case) the large areas of statistical uncertainty surrounding these dates, too, suggests that it is preferable to omit these also for the moment even though no inversions are apparent at these two sites. Their standard deviations are equivalent respectively to ± 400 and ± 300 years.

When all these dates are omitted, seven multi-date sites not showing inversions are represented in this period. These are sites 70, 71, 89, 108, 113, 155 and 173. None of these is however represented by more than one date within or immediately adjacent to the 200 year period under investigation, so they unfortunately cast no further light on whether the dates in Inversion Group II do in fact reflect a perturbation of the radiocarbon input at this period.

This possibility may be supported however by the fact that it can be stated that all multiple-date sites represented by more than one date in or immediately adjacent to this period do in fact show inversions.

This holds true for site 85, as well as those included in Group II. It will be recalled that the contents of Inversion Group II was restricted to sites where the inversions did not seem immediately explicable in terms of contamination or disturbance. Site 85 was accordingly excluded because the submarine peat there exposed on the seabed of the Kattegat is at present being eroded, and because of the durable nature of the flakes of peat it seemed possible that the inversion might be due simply to redeposition. This indeed may be the case, but the dates involved are, top, St 2172, 8010 ± 100 and bottom, St 1820, 7565 ± 110, and St 1820 in particular coincides very closely with St 1532, St 1533 and

and St 1216 within Group II, although St 2172 falls outside the older limit of the Group.

Until dendrochronological data becomes available to confirm or eliminate the possibility of a perturbation, it thus seems unwise to place any reliance on the order in which the radiocarbon "dates" occur in this particular period. Altogether, at 7 sites, 12 dates from peatbeds appear to refer to freshwater stages or actual regressions. These are: Cancale W 705; Karrnamosse St 462; 113 Hv 218; 99, St 1216, St 1233; 85 1820; 77, U 1016, U 489, U 488; 68, St 1532, St 1533, St 1531). On the other hand at 7 sites, each represented by one date, increases of marine influence appear to be indicated. These are: 173, GrN 201; 155, GrN 1123; 108, KI 214; 99, St 1215; 89, St 2011; 71, St 1492; 70, St 1292. The contrast between this balance and the total predominance of evidence of transgression from 8350 to 7750, and from 7500 to 7000 B.P., might be taken to suggest that the minimum on the graphs at 7500 B.P. indicated some degree of separation between Phases C and D. However, on balance it seems preferable to accept that until the Bristlecone Pine calibration of the radiocarbon timescale has been extended to cover this period adequately, the possibility that the proposed Phases C and D are distinct can neither be properly established nor eliminated. With this proviso, henceforth this period will be referred to as Phase C/D.

With the exception of these two and a half centuries, however, the pattern of the evidence between 8550 and 6800 B.P. seems as well defined as that of Phase B.

(ix) Phase E

According to the graphs, this phase starts about 6800 B.P., with evidence of transgression increasing to a maximum at 6400, before falling off until it reaches a minimum at 6150 B.P. The shortage of positive evidence of regression, characteristic of the majority of the periods considered so far, is at this stage replaced by a somewhat more balanced representation of evidence of increases and decreases of marine influence. This Phase thus has more in common with the evidence of the later Holocene. This is also true in another respect, in that since the whole of Phase E lies after 7000 B.P., no question arises of any isolation of the Baltic as a lake, and it seems safe to assume that the Baltic's surface was under the influence of the level of the oceans throughout this period. Including the Baltic evidence, just over 70 radiocarbon dates are available between 6800 and 6000 B.P.

Relatively few of these dates are uninformative. However, Q 35 and TA 55 (both peat), and T 156 (shells) must be disregarded since the stratigraphic contexts of the dates are not sufficiently diagnostic of individual transgression or regression stages for present purposes. These determinations were from single-date sites. Among multiple-date sites, T 121 (dating shells from site 41) is omitted for the same reason. At site 37, a laboratory test (U 2115) confirmed that the age of U 648 was too old due to the presence in the sediment of old carbon, probably graphite. It therefore seems advisable to omit this date altogether.

GrN 1626, 6765 ± 60 from site 178 has already been dealt with in earlier chapters. This is the Class W date that appeared anomalous in terms of the patterns, not only of the Class W but also the Class T, R and I graphs, and which furthermore was the sole cause of an apparent disagreement between the Baltic and non-Baltic curves. Since it is in disagreement with the patterns yielded by so many other radiocarbon dates, it does not appear to be a reliable indicator of changes of the sort under examination here.

Of the remaining 31 multiple-date sites, 6 are represented by 2 dates each within Phase E (sites 64, 77, 79, 96, 152 and 192). In the cases of sites 77 and 79 the pairs of dates each refer only to tests on the same samples. At sites 64 and 192 separate samples are involved in the pairs of dates but there is no implication there of more than one transgression during Phase E.

The stratigraphy at sites 96 and 152 raises this possibility however, and suggests that at these sites at least a subsidiary fluctuation of marine influence occurred in addition to the main transgression shown by graphs. In both cases this appears to have intervened between the general transgression maximum shown by the graphs and 6400 and the succeeding regression maximum ca. 6150 B.P. Among sites with sequences fixed by one date within this Phase together with other dates earlier or later, at least sites 9, 23, and 153 appear to give some support to this additional fluctuation, as do several of the single-date sites. It therefore appears advisable to examine the internal consistency of Phase E in some detail.

First, however, it is necessary to establish whether this Phase may legitimately be considered to be distinct from the previous period.

The stratigraphy at individual sites appears to support this. This is for instance particularly clear at site 120 in Northern Ireland, where distinct transgressions are dated specifically at 7650 and 6550 B.P. Furthermore, this is substantiated by the overall time-distribution of the evidence. As noted when discussing the Phase C/D, the evidence of regression which became dominant at 7000 B.P. continues after 6800. Thus, sites 10 and 11, both the Harmanger area (61° 17°) were isolated from marine influence at St 754, 6780 ± 155 and St 749, 6750 ± 140, respectively, while at Immingham in Lincolnshire (53° 0°) a bed of peat in which alder trees grew intervened between deposits of brackish clay. One of these trees yielded a date of Q 401, 6681 ± 130. An alder forest, also drowned by the sea, grew simultaneously at site 131, Nordenhamn on the German North Sea coast (53° 8°), Hv 104, 6660 ± 120.

Since dates referring to transgression do not begin to accumulate until about 6600 B.P., evidence of regression is thus completely dominant for almost 400 years. Thus not only the graphs of Classes T R W and I but the pattern of the evidence as a whole supports the conclusion that the transgression of Phase E was indeed distinct from the transgression(s) of the period between 8300 and 7000 B.P.

The internal consistency of the evidence from Phase E will now be examined in detail. As noted above, the stratigraphy at some sites suggests a possible secondary fluctuation of marine influence, not shown by the general graphs of Classes T W R and I dates. If this occurred, it was certainly of very short duration (ca 200 - 300 years) and the investigation of the possibility thus involves working very close to the limits of resolution of the radiocarbon dates. Only limited confidence can therefore be felt in any conclusions. Any suggestion of a perturbation of the radiocarbon input curve at this time would make it very difficult to reach any conclusion at all. However, Bristlecone Pine calibration work has recently been extended to cover this period (Ferguson, in Neustupny, 1970) and the most recent note available on the results (Suess, 1970) does not suggest a perturbation at this period. Certainly the writer found no accumulation of unexplained inversions equivalent to Inversion Groups I or II within Phase E. On the contrary, as will now be shown, this Phase is characterised by a high level of internal consistency in the overall pattern of the evidence. This is so both before and after the possible secondary fluctuation.

With the exception of St 774, 6530 ± 150 marking the isolation of site 12 (61° 17°) from the Baltic, Hv 104 (quoted above) is the last evidence of any increase in marine influence until after the maximum of the graph at 6400 B.P. Otherwise, between 6650 and 6400 evidence of transgression is completely dominant.

Transgressed peat on Gotland Island ($57^{\circ}18^{\circ}$) dates from 6630 ± 80

(St 1582) at site 63, Snoder, and from 6615 ± 100 (St 1620) at site 64, Helgmyr, while further south, on the Swedish mainland at Ostra Landborgen (56° 16°, site 77) the top of a peat bed under marine clay dates from 6570 ± 180: U 485 (with different pretreatment the same sample yielded 6410 ± 110: U 571).

Simultaneously, at Westward Ho in Devon (51° 4° W) the top of a peatbed accumulated on the foreshore over a Mesolithic midden is dated Q 672, 6585 ± 130. Detailed work by D.M. Churchill confirmed that this sample did indeed represent the youngest obtainable date prior to submergence.

Meanwhile at site 120 in Northern Ireland (54° 5°W) marine clay from a transgression contact gave a date of 6550 ± 300, while at Breifjell in Norway (59° 10°) shells referred to by Olaf Holtdahl to a transgressive phase yielded a date of 6545 ± 195: T 123. At site 167 at Willemstad in the Netherlands (51°4°) the effects of a watertable rising under marine influence became apparent at 6525 ± 250 : GrN 240, and at Ekilstorp, site 86 on the Swedish Kattegat coast (56° 12°) marine sand and silt buried a brushwood peat at St 1711, 6520 ± 105.

In the Baltic at Ovra Sandby (site 79, 56° 16°) peat was overlaid by a shore deposit at 6510 ± 110, U 495 (the date of U 495, 6910 ± 120 from the same sample after an alternative pretreatment appears anomalous). Peat just preceding the onset of the transgression at site 112, Husum (54° 9°) on the German North Sea coast yielded a date of 6500 ± 140: Hv 222, while at Launesmyra in west Norway (58°7°) shells attributed by Gunnar Gabrielsen to a transgressive phase dated from 6495 ± 195: T 292.

At 6475 ± 70: GrN 1617, a peat bed at Nieuwe Wetering (site 152 52° 4°) in the Netherlands was buried by marine clay, while at 6460 ± 145:GrN 621 (site 142 53° 6°) at Farmsum was affected by a watertable rising under marine influence.

Meanwhile, on Gotland Island in the Baltic (57° 18°) peat underlying marine sediments yielded dates of St 1589, 6470 ± 120 at site 62, Mallingsmyr, St 1619, 6450 ± 100 at site 64, Helgmyr, and St 1612, 6415 ± 80 at site 61, Dynisse. At this time transgression contacts were registered in the Oresund area (56° 12°, site 96) at St 2510, 6450 ± 100, and at a site near Ekilstorp nearby in Sweden (also 56° 12°) at St 1819, 6430 ± 140.

The situation after 6400 contrasts with the complete dominance of evidence of transgression that prevails immediately before that date. After GrN 1621, 6390 ± 85 at Alphen (site 150 52° 4°) and GrN 1627, 6380 ± 85 at Middelburg (site 178, 51° 3°), there is a general shortage of clear-cut transgression contacts, and although rises of watertable continue to be reflected in basal peats on the east coast of the North Sea, evidence of regression begins to build up over a wide front.

Such basal peats are represented at Waarde (site 166, 51° 4°, GrN 1112, 6370 ± 85), near Husum (54° 9°, Hv 224, 6350 ± 80) and at Koegras (site 154, GrN 455, 6320 ± 185). The first indications of regression are the isolation of site 9 (61° 17°) from the sea at St 715, 6375 ± 90, and the retreat of the sea from a shoreline in north Norway, leaving stranded driftwood which was preserved by the growth of peat on the abandoned beach. The last driftwood at Djupdalen in Finmark (71° 24°) has been dated 6350 ± 150: T 185.

The occurrence of a brief regression at this time is demonstrated by the occurrence of a widespread thin peat bed in the Old Tidal Flat deposits of Holland. At Honselersdijk (52° 4°, site 153) this has been dated 6330 ± 150, GrN 639, and at Nieuwe Wetering (52° 4°, site 152), 6320 ± 70: GrN 1620.

Dates from a comparable stratigraphic context at Fawley in Hampshire (site 192, 50° 1°) bracket the Dutch dates (Q 834, 6366 ± 124, and Q 835, 6318 ± 134), while a fall of relative sea level at Virolahti in Finland (site 23, 60° 27°) also coincides closely with these dates (Su 65: 6310

$\pm$ 130). It is matched by the isolation of site 10 (61° 17°) from the sea, followed shortly afterwards by site 11 nearby, St 753, 6295 $\pm$ 185 and St 710 6260 $\pm$ 100 respectively.

A further driftwood site at Nord-Mjele (69° 15°) in Arctic Norway may also correspond to these other indications of regression. Marcus Marthinussen however encountered there local problems of interpretation not present at his other site at Djupdalen, so this date may not be so reliable geologically. Certainly the mean of the determination, T 267, 6250 $\pm$ 200, like St 710 overlaps slightly with the means of some of the dates which apparently indicate that a brief if widespread transgression succeeded the regression phase just described.

Rising coastal watertables were registered at Ternaard (site 144, 53° 6°) GrN 606, 6295 $\pm$ 140, Perkpolder (51° 4°) GrN 1045, 6240 $\pm$ 70 and St. Maartensvlotbrug (site 156, 52° 4°) GrN 1633, 6200 $\pm$ 100, but as noted above no weight can be put on this since these effects occurred throughout the period of interest here.

The occurrence of a second transgression within Phase E is best documented in the Kattegat area. Morner (1969) has drawn attention to the importance of the low tidal range there, now and in the past, for facilitating the detection of minor fluctuations of sea level. His main "PTM 3" ("Postglacial Transgression Maximum 3") coincides closely with the present writer's maximum of Phase E transgressive evidence at 6400 B.P. However, he finds enough evidence of a double transgression maximum (PTM 3A and 3B, about 6450 and 6250) to feel justified in showing both not only among his local results but also on his curve of eustatic ocean level variations (illustrated and discussed in Chapter 12).

He supports the distinction on both morphological and stratigraphic grounds. The radiocarbon dates at site 96 in Oresund (56° 12°) were intended to confirm this separation, and in fact they conformed closely to prediction.

PTM 3A was dated by St 2510 (already given above, 6450 ± 100) and PTM 3B by St 2527, 6275 ± 100.

Although this particular possibility has not been investigated as specifically elsewhere, there are some indications that Morner may be correct in suspecting a small eustatic, or at least widespread, transgression about 6250 B.P.

The notable thinness of the peat layers just mentioned at sites in the Dutch Old Tidal Flats and at Fawley may be consistent with a very short period of growth, perhaps (on the analogy of evidence later in the Holocene) related to the onset of transgression. Furthermore, three other radiocarbon dates falling at this time in areas widely separated from the Kattegat could be taken to indicate a rise in sea level. U 79, 6230 ± 110 in Arctic Norway at Elvegard (68° 17°) refers to a treetrunk dating a sand layer which may reflect the effect of a marine transgression in a delta sequence. The other dates are from Somerset, at Bridgewater Bay and Burnham-on-Sea (both 51° 3°W). NPL 148, 6230 ± 95 at Bridgewater refers to the top of a peat bed, where this is covered with marine mud. The top of the bed may have been eroded, however, so this is not necessarily a true transgression contact. However, at Burnham, Q 134, 6262 ± 130 dates peat exposed on the present sea bed. Since the pollen analyses show that this peat was formed in brackish conditions its date would certainly appear to reflect the drowning of the bed by the sea.

It is not clear what interpretation should be put on the accumulation of "peaty clay" and "clayey peat" shortly afterward at sites 194 and 131, (Picardie, Gif 398, 6200 ± 300, Saxony, Hv 103, 6200 ± 175, respectively). However, from then onwards evidence of regression once more becomes dominant. Indeed no suggestion of transgression is again apparent until 6050 B.P.

The first sure sign of regression after 6250 is apparent at Margam in Wales (site 184, 51° 3°W). There Q 275, 6184 ± 143 dates the base of a peat bed on silty marine clay, where it shows a transition from brackish to freshwater conditions. Then Su 31, 6170 ± 160 from Pello (site 7, 66° 24°) in Finland dates a regression stage defined by pollen analysis. At the same time a site at Kalltorpmossen (59° 18°) near Stockholm was isolated from the sea, St 788, 6170 ± 110.

On the German North Sea coast at Delve (site 109, 54° 9°), the growth of Phragmites peat marked the end of a transgression at 6130 ± 85, Hv 629, while a thin peat layer intervening between beach deposits at Ramsa (site 5, 69° 16°) in Arctic Norway placed the regression there at 6100 ± 150: T 296.

Three further dates indicating regression occur prior to 6000 B.P. Of these, Q 380, 6026 ± 135 refers to birchwood that grew on blue marine silt shortly after a regression at Ynyslas (site 157, 52° 4°W) in Wales. Su 59, 6010 ± 105 dates Phragmites material associated with the cessation of transgression at Porvoo (site 24, 60° 25°) in Finland, and St 719, 6000 ± 140 indicates the isolation of site 12 (61° 17°) on the other side of the Baltic.

However, as noted above, from 6050 B.P. onwards transgression evidence begins to build up. This will be tabulated in Part II since it relates to the onset of Phase F.

On balance, then, it would seem that following a general regression between 7000 and 6650 B.P., Phase E began with a distinct transgression that reached a meximum at 6400 B.P. This may have been succeeded immediately by a regression, terminated by a further slight transgression about 6250 B.P., before regression again set in.

As noted in the Introduction, the conclusions from this chapter will be summarised along with those from Chapters 9 and 10 in the opening section of Chapter 11.

Figure 8.1 Identification of Phases prior to 6000 B.P. in terms of the Difference Curve.

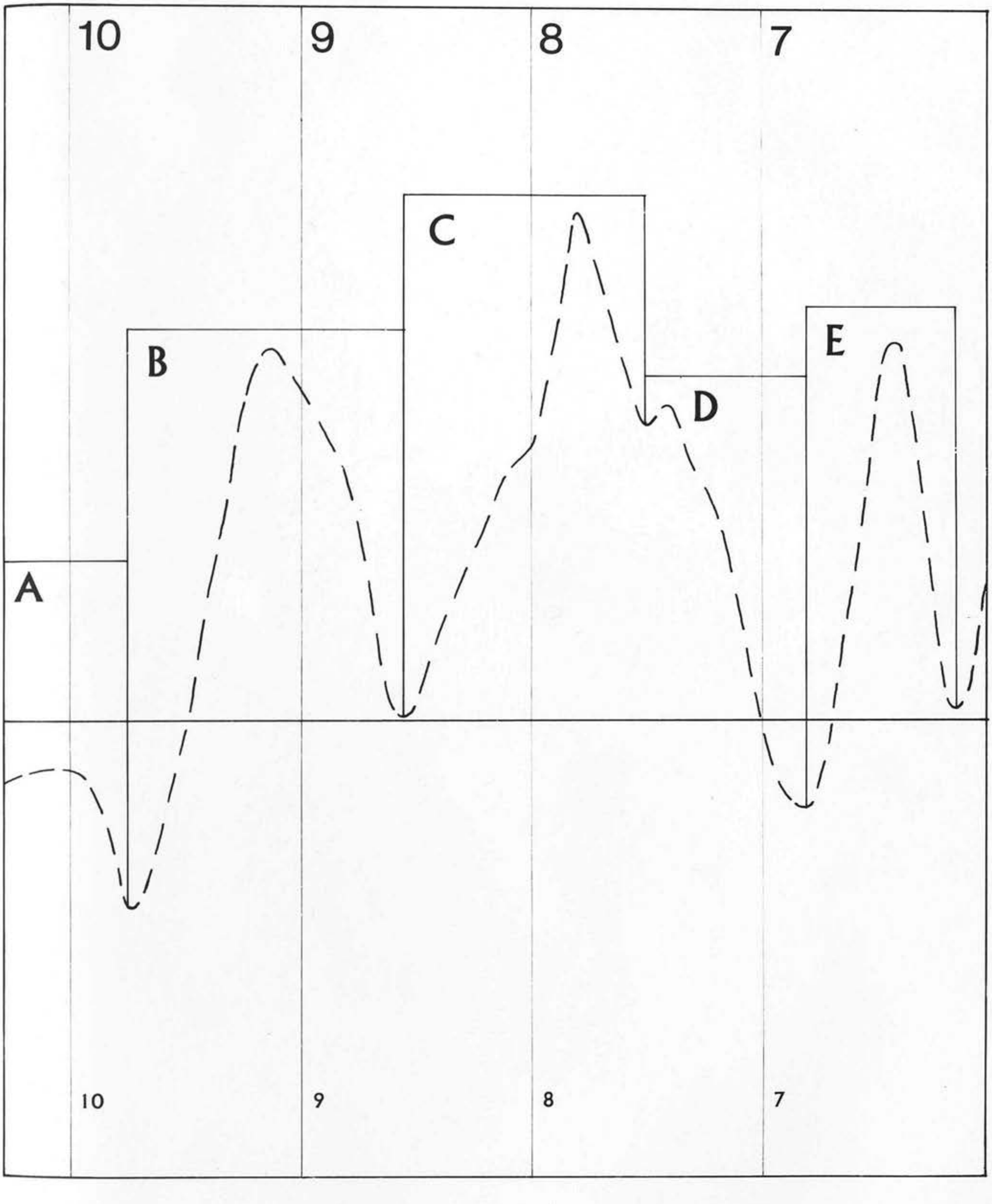
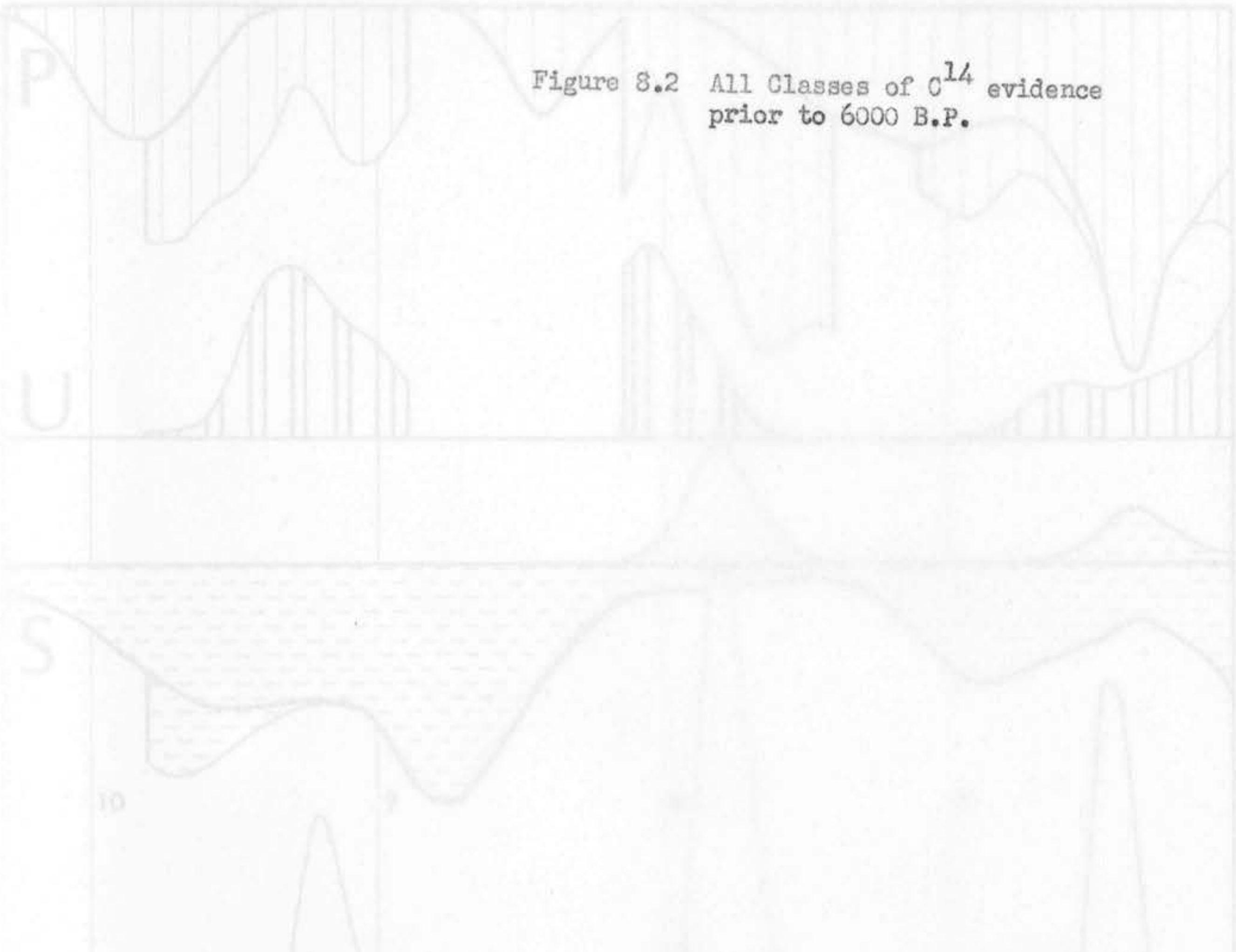
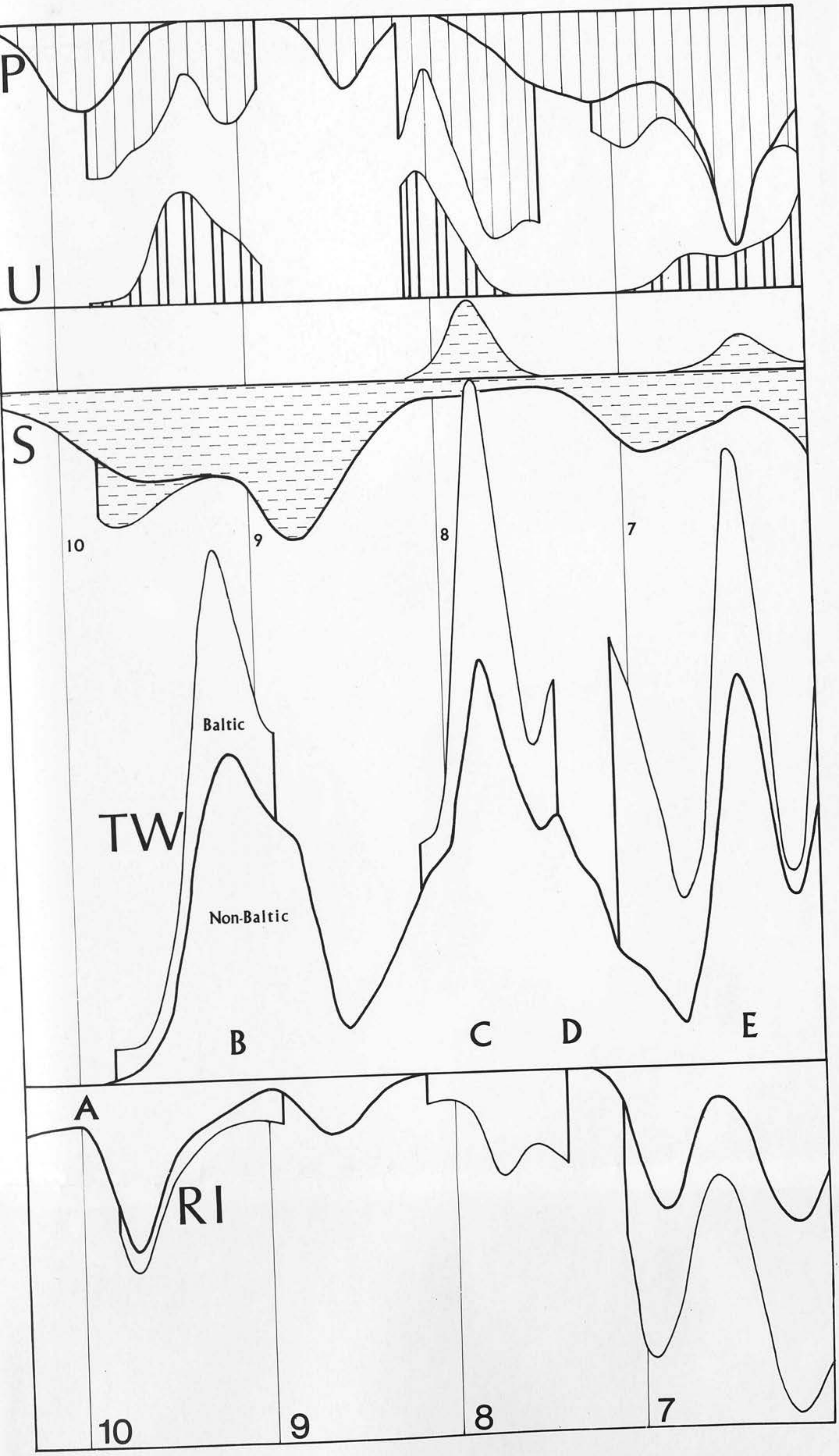

10
9
8
7
A
B
C
D
E
10
9
8
7

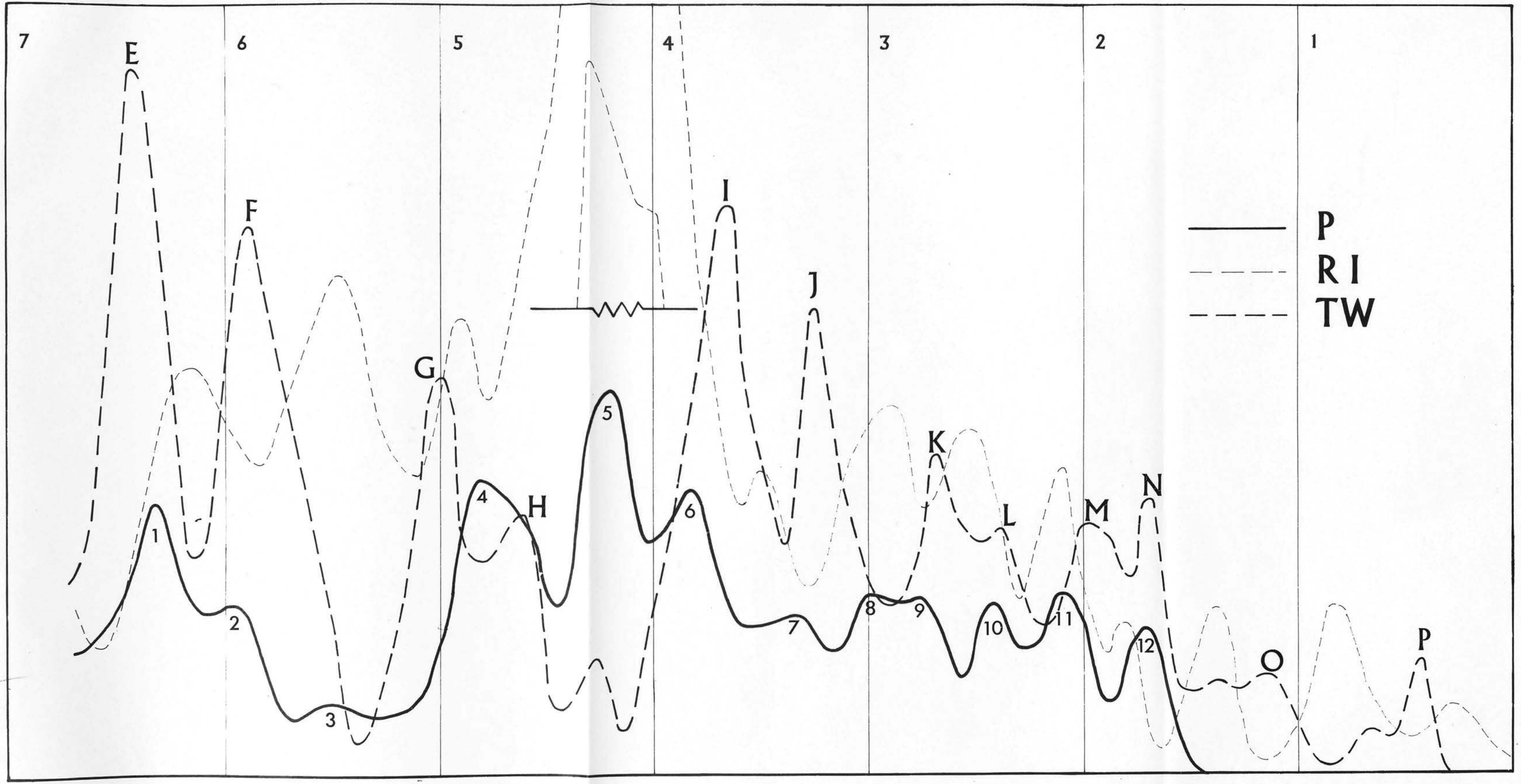
Figure 8.2 All Classes of C^{14} evidence
prior to 6000 B.P.





P
U
S
10
9
8
7
Baltic
TW
Non-Baltic
B
C
D
E
A
RI
10
9
8
7

Figure 8.3 The relationship between the curve for Class P C^{14} dates, and those for Classes TW and RI.


7
6
5
4
3
2
1
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F
G
H
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K
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12
P
R I
TW

Chapter 9

The overall pattern of the radiocarbon dated evidence from the western seaboard of Europe, excluding Scotland.

Part II: 7000 to 3000 B.P.

Introduction

In section (i) of this Part, the phases of transgression represented by the Class T W R I graphs are identified and their general characteristics are described. Then in (ii) the relationships between these classes and the overall patterns of the other main categories of evidence (Classes P, U and S) are examined. In (iii) the date sequences at multiple date sites are identified, and radiocarbon determinations of limited value in the present context are indicated for the whole of the period covered in this Part. Phases F, G and H are then examined individually in (iv) to (vi) respectively, while (vii) deals with the special case of the period between Phases H and I. As in the cases of Parts I and III, the results from this Part are summarised at the start of Chapter 11.

(i) The graphs used in formulating the initial hypothesis suggest six transgressive phases between 7000 and 6000 B.P. These are identified in terms of the Difference Curve, in Fig. 9.1 (E to J), and illustrated more fully in Fig. 9.2.

Phase E has already been considered. It will be recalled that it reaches its peak at 6400 B.P. before being replaced at 6150 B.P. by Phase F. In marked contrast to the earlier period of the Holocene considered in Part I, after Phase E the balance of the evidence shifts from dominance by evidence of transgression to dominance by evidence of regression. The latter tendency is evident in an extreme form between circa 4600 and 3800 B.P., but from then on until the present day the amounts of the two types of evidence are, in general, more evenly balanced than in either of the previous parts of the Holocene.

Phases F, G and H all reflect the trend towards evidence of regression that is apparent in the two millennia after 6000 B.P. Although all are marked by peaks of positive (T W) evidence of transgression, these peaks decrease in size, and all are smaller than than of Phase E. Taken together with the overall increase in Class R I evidence, the effect of this is to shift the Difference Curve towards the regression side of the abscissa. Thus although the regression peak following Phase E was smaller than the transgression peak of that Phase, the reverse is the case of Phase F, while the Difference Curve hardly crosses the abscissa at the peak of Phase G, and fails even to reach it at the peak of Phase H. The ensuing regression maximum, between Phases H and I, is then by far the largest feature of the Class T W R I graphs in the entire Holocene. Phases I and J then however show a return of the Difference Curve to the transgression side of the abscissa.

It will be seen that, according to the graphs, the duration of these Phases is as follows:-

Phase	Starts	Peak	Ends
E	6800	6400	6150
F	6150	5900	5400
G	5400	5000	4900
H	4900	4750	4300
I	4300	3650	3450
J	3450	3250	2900

The evidence falling after the peak of Phase I will be dealt with mainly in Part III, which deals with the period from 4000 B.P. to the present day, and in this Part attention will be concentrated on the proposed Phases F to I.

(ii) Altogether a total of 447 radiocarbon dates are available for the period between 7000 and 4000 B.P. Fig. 9.2 shows the overall patterns of the evidence of Classes T W, R I, S, U and P. The presentation is handled in the same way as in the equivalent diagram in Part I (Chapter 8) except that (as noted there) the Class P graph is no longer shown inverted. Because the restrictions of the thesis format make it impracticable to show the peak of R I evidence between Phases H and I to the same scale as the rest of the diagram, between 5000 and 3000 B.P. R I and Difference Curves recalculated to one quarter of the general vertical scale are superimposed on the graph. These are distinguished by a lighter line.

It will be recalled from Part I that Phase E is represented by maxima of the T W, P and transgressive S curves; that it is immediately preceded by a small maximum of the U curve; and that corresponding minima of the R I and general S curves are registered at its time.

Phase F is also represented by the majority of the Classes of evidence. As well as registering a corresponding maximum and minimum in the T W and R I curves, it is also reflected by peaks in the graphs of Classes P and U. No shell evidence clearly attributable to conditions of increasing marine influence is available at this period. The peak of regressive evidence between F and G (viz. R I curve) is however marked by a considerable accumulation of shell bed dates (general S curve), as well as by decreases in the P and U evidence.

Phases G and H are reflected separately in the T W and R I curves, and the U curve shows a maximum which coincides with the peak of G but not of H. However, the maximum of the P curve and corresponding minimum of the general S curve at this time appear to embrace both G and H as a unit, without differentiating between them.

As noted above, the largest Holocene accumulation of Class R and I evidence occurs between H and I. The size of this accumulation was such that it altogether outweighed the small peak of evidence of transgression exhibited at at ca. 4250 B.P. by the T W curve. An initial decrease in the evidence of regression occurred at this time (R I curve) however, and as the quarter scale graph shows (on Fig. 9.2), this combination of changes in the evidence was reflected in an abrupt truncation of the regression peak of the Difference Curve between 4300 and 4150 B.P.

On the full scale graph (Fig. 9.3) there is indeed some indication of a small inflexion towards transgression in that section of the Difference Curve, but this was so slight (and the imbalance in favour of evidence of regression was so great at that period) that it was not considered that it would be justifiable to distinguish 4300 to 4150 as a distinct phase, at the level of the initial hypothesis.

That this possibility requires further investigation is however suggested by the fact that the largest peak of Class P evidence encountered in the Holocene is centred at 4200 B.P. It is also notable that the step at 4100 - 4000 B.P. in the R I curve before this falls away with the onset of Phase I, is consistent with the concept of a brief alternation of transgression with regression in some areas between about 4400 and 4100 B.P. This will therefore be examined further.

As indicated above, Phases I and J will be considered in detail in Part III.

(iii) Multiple-date sites represented between 7000 and 3000 B.P.

70°	24°	1	T 184, T 183
69°	16°	4	T 380, T 353
68°	13°	6	U 98, U 160, U 99
61°	17°	12	St 774, St 719, St 781
		13	St 769, St 718
		14	St 755, St 713
		15	St 773, St 711

	16	St 770, St 716
	17	St 775, St 714
	18	St 747, St 708
60° 28°	22	Su 16, Su 11
60° 25°	24	Su 53, Su 41, Su 62, Su 59, Su 57, Su 58, Su 53
	25	Su 64, Su 52
60° 17°	26	U 452, U 471
	27	U 472, U 642, U 2024
	28	U 225, U 2025, U 704
	29	U 226, U 454, U 455
	30	U 2109, U 639, U 664
	31	U 451, U 663, U 2089, U 2073
59° 17°	33	U 2046, U 2047
	34	U 439, U 438
59° 16°	35	U 436, U 430, U 435
	36	U 1004, U 432, U 433, U 1005
	37	U 2022, U 648, U 2055, U 522
59° 14°	39	U 2012, U 606, U 513, U 2021
59° 10°	41	T 117, T 121
	42	T 119A, T 119B
	44	T 91A, T 91B
58° 16°	48	U 43, U 41
58° 11°	52	Lu 141, Lu 137, Lu 136
58° 7°	54	T 209, T 281
57° 18°	61	St 1609, St 1612, St 1617
	62	St 1588, St 1589
	63	St 1583, St 1582, St 1584, St 1585

		64	St 1622, St 1620, St 1619
		65	St 1591, St 190
57°	12°	67	St 1893, St 1890, St 1891
		70	St 1292, St 1291
57°	10°	75	K 864, K 865
		76	K 906:1, K 906:2, K 890:1, K 890:2, K 907, K 888, K 889, K 902
56°	16°	77	U 489, U 485, U 571
		79	U 486, U 514, U 495
56°	15°	80	Lu 187, Lu 147
56°	12°	86	St 1711, St 2214, St 1815
		87	St 1818, St 2177
		92	St 2203, St 2176
		94	St 2287, St 2345, St 2286, St 2336, St 2285
		97	St 2894 dup., St 2893
54°	11°	105	KI 217, KI 216
54°	9°	109	Hv 622, Hv 629, Hv 631
		110	Hv 764, Hv 766
		111	Hv 183, Hv 181, Hv 179, Hv 176
		112	Hv 225, Hv 222, Hv 226, Hv 238, Hv 228, Hv 229, Hv 231
		113	Hv 218, Hv 219, Hv 220
		114	Hv 241, Hv 242, Hv 243
54°	8°	115	K 796, K 795, K 797
		116	KI 95, KI 100, KI 200, KI 201
		117	KI 203, KI 98
		118	KI 97, KI 202, KI 99
		119	KI 232, KI 233, KI 234, KI 231
54°	5°W	120	LJ 904, LJ 903, LJ 908, LJ 907

		121	Q 632, Q 770, Q 633, Q 635
		122	I 1198, I 1199
54°	2°W	123	Q 261, Q 256, Q 260
53°	10°	125	Hv 561a, Hv 662
		126	Hv 309, Hv 308
53°	9°	127	Hv 30, Hv 27, Hv 28, Hv 26
53°	8°	128	Hv 164, Hv 163
		130	Hv 100, Hv 99, Hv 98
		131	Hv 104, Hv 103, Hv 102, Hv 101
		132	Hv 111, Hv 110, Hv 109, Hv 108, Hv 107, Hv 106, Hv 105
		133	Hv 290, Hv 289
		134	Hv 292, Hv 291
53°	7°	135	Hv 545, Hv 544, Hv 542, Hv 543
		136	Hv 890, Hv 889
		138	Hv 248, Hv 247, Hv 246, Hv 245
		139	Hv 41, Hv 40, Hv 39
		140	GrN 1091, GrN 1088, GrN 1090, GrN 1089
53°	6°	142	GrN 621, GrN 637, GrN 619
		143	GrN 655, GrN 657, GrN 656
		144	GrN 606, GrN 601, GrN 499, GrN 602
53°	6°W	145	D 4260, BM 78
53°	5°	146	GrN 605, GrN 610, GrN 609, GrN 617
53°	3°W	147	Q 620, Q 620 dup.
53°	0°W	149	Q 685, Q 686, Q 688
52°	4°	150	GrN 2619, GrN 1621, GrN 1622
		151	GrN 1057, GrN 1650, GrN 1649
		152	GrN 1618, GrN 1617, GrN 1620

		153	GrN 639, GrN 633
		154	GrN 455, GrN 1060, GrN 476, GrN 460, GrN 463
		155	GrN 1123, GrN 1131
		156	GrN 1633, GrN 1135
52°	4°W	157	Q 380, Q 382, Q 712
52°	0°	158	Q 532, Q 531
		159	Q 130, Q 129
		160	Q 581, Q 580, Q 499
52°	0°	161	Q 263, Q 264
		162	Q 489, Q 490, Q 550
51°	4°	164	GrN 1098, GrN 1096, GrN 1095
		165	GrN 315, GrN 308
		166	GrN 1112, GrN 348, GrN 1113, GrN 346
		167	GrN 240, GrN 228, GrN 222, GrN 238, GrN 256, GrN 220
		168	GrN 1143, GrN 1139, GrN 1142
		169	GrN 286, GrN 296
		170	GrN 1042, GrN 1041
		171	GrN 1048, GrN 1049
		173	GrN 186, GrN 203, GrN 189, GrN 191, GrN 192
		174	GrN 1160, GrN 1151, GrN 1140, GrN 1146, GrN 1144, GrN 1141, GrN 1148
		175	GrN 294, GrN 292
51°	3°	176	GrN 1036, GrN 1039, GrN 1035
		177	GrN 1580, GrN 421, GrN 417
		178	GrN 1626, GrN 1627
		179	GrN 405, GrN 385
		180	GrN 1571, GrN 1566, GrN 329, GrN 332
		181	Lv 134, Lv 131, Lv 130, Lv 133

51° 3°W 182 I 2688, I 2689

183 NPL 148, NPL 147, NPL 146

184 Q 275, Q 274, Q 265

51° 2°W 186 Q 120, Q 126

51° 0° 188 Q 810, Q 811

189 Q 791, Q 790, Q 790 dup.

190 Q 792, Q 792 dup.

50° 2°W 191 I 3431, I 3429

50° 1° 192 Q 834, Q 835, Q 831, Q 832

193 Gif 396, Gif 397

194 Gif 398, Gif 399

49° 0°W 198 Gif 728, Gif 371

48° 1°W 199 Gif 390, Gif 391

48° 4°W 200 Gsy 47B, GrN 1966, Gsy 47A, Gif 159, Gif 47C, Gif 278, Gif 160

47° 2°W 201 Gif 193A, Gif 193B

202 Sa 39, Sa 41, Sa 46, Sa 42, Sa 35, Sa 40, Sa 43

318 of the 447 radiocarbon dates available for the period between 7000 and 3000 B.P. come from sites represented by series of dates rather than individual determinations. In all, 131 of these multiple-date sites are involved.

The occurrence within this period of perturbations in the curve of radiocarbon input has been discussed in a previous chapter. Five inversions or possible inversions of date order relative to stratigraphic order are represented between 7000 and 3000 B.P. These are at sites 33, 94, 115, 123, and 186.

Those at sites 33 and 115 were thought to be due to later disturbance of these sites. The determinations involved do not coincide with the perturbations of the radiocarbon curve established by Bristlecone Pine calibration so it seems safe to accept this. At site 123 (Silverdale) the cause of the confusion is unknown (Oldfield, 1960), but again the determinations do not coincide with the known perturbations.

It was not clear from the reports whether any inversion did in fact occur at site 186 (Tealham), though the dates there (5620, 5412) do bracket the known perturbation at 5550 B.P. An otherwise unexplained inversion did definitely occur at site 94 (Torekov) however, at a time coinciding with the further perturbation at 4950 B.P. (5025 ± 100, 4890 ± 100), and it would seem that this may reasonably be attributed to the radiocarbon variations.

This is however only one out of 131 multiple-date sites, and involves only two out of 318 radiocarbon determinations coming from stratigraphically linked sequences between 7000 and 3000 B.P. Furthermore, no inversions at all are apparent between 4450 and 3750 B.P., when the calibration work shows several rather small perturbations. It would thus seem that these variations are not of importance at the level of the present enquiry.

In addition to the 5 sites involved in inversions, it would seem preferable to disregard 11 other multiple-date sites because some of their dates may be anomalous due to problems encountered in sampling and the possibility of contamination (for instance, either by younger roots or by ancient lime). These are sites 27, 28, 29, 35, 36, 37, 39, 44, 86, 162 and 192. Six further sites are of limited value because their stratigraphy is uninformative (or its interpretation unclear) in the present context. These are sites 67, 145, 146, 169, 181, and 200. In all, this leaves 109 multiple-date sites. The evidence of the sequences they display will now be used as the starting point for the examination of each proposed Phase.

It may be added here that at single-date sites the precise interpretation of 10 further determinations in terms of transgression or regression is unclear. Four of these refer to shell beds (Gif 390, K 864, K 865, K 900) and five to botanical samples (GrN 657, Q 129, Sa 40, Sa 46, St 1591). The final one, St 2488, refers to the skeleton of a man who was murdered with a stone axe and thrown into the Kattegat when its level was different from at present. Five further dates demonstrate only that land stages were in progress at sites subject to marine transgression (BM 78, Gif 282, Gsy 47A, Lv 130, Lv 131, Lu 133).

(iv) Phase F

Phase E has already been examined in Part I. Phase F lasts between 6150 and 5400 B.P., according to the Difference Curve, and altogether some 60 radiocarbon dates are available for this period.

33 multiple-date sites are represented within the period of Phase F. Sites 24 and 76 are represented by four dates each and sites 1, 6, 12, 13, 112, 157, 173 and 174 all by two dates each. The remaining sites, represented by one date within this phase and others earlier and later are sites 8, 14, 15, 22, 61, 63, 105, 109, 111, 132, 150, 155, 167, 168, 170, 171, 179, 180, 184, 188, 189, 190 and 198. In none of these cases is there any clear indication that more than one transgression occurred during the period designated Phase F.

The overall pattern of the radiocarbon dates falling between 6150 and 5400 B.P. is a clear one. It will be recalled from the discussion of Phase E in Part I that evidence of regression became dominant at 6250, and no further evidence of transgression was apparent until 6050 B.P. With the exception only of Q 380, Su 59 and St 719, noted in Part I (all of which fell just before 6000 B.P.) evidence showing transgression then becomes dominant from 6050 to ca 5900 B.P. The determinations involved are:

51°	4°	173	GrN 203	6050 ± 200	W
54°	9°		Hv 232	6050 ± 170	T
53°	8°	132	Hv 111	6050 ± 140	W
51°	4°	167	GrN 228	6040 ± 130	W

55° 9°		K 1222	6035 ± 130	S
68° 13°	<u>6</u>	U 98	5990 ± 110	T
51° 4°<u>W</u>		NPL 113	5970 ± 90	U
51° 5°W		Q 530	5960 ± 120	U
58° 24°		TA 183	5950 ± 60	M
51° 4°	<u>174</u>	GrN 1160	5945 ± 90	W
54° 11°	<u>105</u>	KI 217	5940 ± 75	M
54° 9°	<u>112</u>	Hv 226	5930 ± 90	T

(In addition, U 160 and Hv 238 provide checks on U 98 and Hv 226 respectively.)

From then on, although rises of watertable continue to be registered in the Netherlands until 5550 (GrN 405, 1042, 1048, 1151, 1571), with the sole exception of one date for the top of a peat bed at site <u>155</u> (GrN 1131, 5550 ± 65) no further transgression contacts or possible contacts are registered between 5900 and 5400 B.P.

Evidence of regression dominates this period. In this case, the determinations are:

59° 17°		St 466	5935 ± 110	I
52° 4°<u>W</u>	<u>157</u>	Q 382	5898 ± 135	R
61° 17°	<u>12</u>	St 781	5890 ± 160	I
60° 25°	<u>24</u>	Su 57	5860 ± 200	M
61° 17°	<u>13</u>	St 769	5810 ± 140	I
51° 0°	<u>189</u>	Q 791	5790 ± 120	P
61° 17°	<u>14</u>	St 755	5745 ± 150	I
70° 24°	<u>1</u>	T 184	5700 ± 150	R
49° 0°<u>W</u>	<u>198</u>	Gif 371	5680 ± 250	R
51° 4°	<u>173</u>	GrN 189	5665 ± 200	R
60° 28°	<u>22</u>	Su 11	5620 ± 140	R

51°	2°W	186	Q 126	5620 ± 120	R
61°	17°	8	St 712	5590 ± 90	I
60°	25°	24	Su 58	5570 ± 200	M
60°	25°	24	Su 53	5560 ± 140	M
51°	0°	188	Q 811	5530 ± 100	P
51°	2°W		Q 423	5510 ± 120	R
70°	24°	1	T 183	5500 ± 150	R
61°	17°	13	St 718	5495 ± 115	I
54°	9°	111	Hv 181	5480 ± 135	R
51°	4°	168	GrN 1143	5420 ± 60	R
61°	17°	15	St 773	5415 ± 180	I
51°	2°W	186	Q 120	5412 ± 130	R
50°	2°W	191	I 3431	5410 ± 115	R

The heavy dominance of regression evidence persists until 5300 B.P. Although the data after 5400 belong nominally to Phase G, in the interests of continuity they will also be listed here.

54°	5°W	121	Q 770	5380 ± 120	M
53°	8°	131	Hv 102	5350 ± 130	P
57°	10°	97	St 2893	5315 ± 100	R
55°	12°	76	U 48	5310 ± 210	M
53°	6°W	145	D 38	5300 ± 170	M

Phase F is thus separated from both the preceding and succeeding Phases by periods dominated by evidence of regression. The period between E and F is essentially clear of evidence of transgression for the 200 years between 6250 and 6050, and that between F and G by the 500 years between 5900 and 5400 B.P. These divisions seem particularly well exemplified in the stratigraphy of sites 12, 24, 61, 63, 150, 152 and 153, and sites 24, 63, 111, 131 and 168, respectively.

(v) Phase G

According to the Difference Curve, this Phase runs from 5400 to 4900 B.P. 45 radiocarbon determinations are available in all.

20 multiple-date sites are represented within this period. Site 76 is represented by three dates, and sites 25, 131 and 154 by two dates each. Those with one date within this period and others earlier or later are 34, 63, 97, 121, 139, 140, 142, 143, 144, 160, 166, 171, 174, 183, 188, and 190. At none of these sites does it appear that more than one transgression occurred during Phase G.

In considering Phase F the dominance of regression evidence after 5900 was noted, as was the fact that this situation continues unbroken until about 5300 B.P. It will be recalled that dates belonging to this regression, but nominally falling within Phase G (taken to begin at 5400 B.P.) were listed with Phase F. These were Q 770, St 2893, U 48 and D 38.

It may be noted here that although the evidence is not conclusive, certain Danish shell beds which have been attributed to a regression phase by Morner (1969) in fact yield dates which coincide with the end of this period of regression evidence. These shell dates are from two sites, Melholt (site 76, 57° 10°) and Lille Vildmose (K 866, 56° 10°).

76	K 888	5315 ± 165
	K 866	5275 ± 175
76	K 889	5255 ± 165
76	K 902	5245 ± 175

A seaweed peat at Bjorkang (57° 12°), St 2331, 5242 ± 125 also appears to indicate regression conditions. There is one Class R date from site 167 as late as 5200 ± 120 (GrN 222), but then no others until 4990 B.P.

It is in this gap in the Class R evidence that the peak of the Phase G transgression evidence occurs. Determinations referring to increases of marine influence form a group between 5300 and 4900 B.P. The most definite of these (Classes T and W) are:

53°	7°	139	Hv 41	5280 ± 100	W
53°	6°	142	GrN 637	5250 ± 120	W
57°	18°	63	St 1585	5240 ± 85	T
52°	0°	160	Q 581	5130 ± 120	T
51°	3°		GrN 187	5100 ± 80	W
53°	7°	140	GrN 1091	5050 ± 65	W
51°	4°	171	GrN 1049	5040 ± 60	T
51°	4°	174	GrN 1140	5030 ± 70	W
53°	6°	143	GrN 655	4995 ± 90	T
50°	2°W	191	I 3429	4980 ± 120	T
48°	4°W		Gsy 75	4980 ± 120	U
53°	6°	144	GrN 601	4975 ± 170	T
52°	4°	154	GrN 1060	4960 ± 80	T
52°	4°	154	GrN 476	4925 ± 190	W

Although no Class R dates fall between 5200 and 4990 B.P., at four sites there are indications that decreases of marine influence took place during the Phase G peak of evidence of transgression. It is notable however that all these sites are in the Scandinavian areas of isostatic uplift (Su 64 is from Finland while the others are in fact all from neighbouring sites around Stockholm).

25	Su 64	5110 ± 115	M
	St 789	5200 ± 130	I
	St 970	5160 ± 100	I
	St 809	5020 ± 80	I

From the time of the last of the dates indicating transgressions, regression evidence begins to build up, and then briefly becomes completely dominant once more. Again, in the interests of continuity, all dates referring to this period of regression will be listed here, although those after 4900 nominally fall within Phase H. These are:

59°	17°	34	U 439	4990 ± 260	I
53°	10°	126	Hv 309	4990 ± 70	R
59°	10°		T 90	4980 ± 160	I
60°	25°	25	Su 52	4940 ± 140	M
51°	4°	166	GrN 1113	4920 ± 55	R
53°	8°		Hv 288	4910 ± 80	R
57°	12°	70	St 1291	4890 ± 90	I
52°	4°	150	GrN 1622	4880 ± 80	R
59°	17°	34	U 438	4820 ± 150	I
70°	23°		T 126	4820 ± 160	R
59°	18°		St 968	4800 ± 110	I

(vi) Phase H

According to the Difference Curve, this Phase runs from 4900 to 4300 B.P., with its peak at 4750 B.P. Altogether, 90 radiocarbon dates are available for this period.

34 multiple-date sites are represented within Phase H, seven of these by two dates each and the remainder by one date within this period and others earlier and later. Those with two dates are sites 30, 65, 132, 138, 159, 160, 168, and the others are sites 15, 16, 34, 105, 109, 112, 113, 116, 127, 130, 134, 144, 146, 150, 151, 155, 156, 161, 164, 166, 167, 170, 173, 174, 175, 179 and 180.

Only at one of these sites (132) is there any indication that there may have been more than one transgression during the period designated Phase H, and at that site the evidence is not conclusive since the samples concerned were not superimposed stratigraphically. It thus seems legitimate to conclude that only one transgression took place during Phase H.

According to the T W and R I graphs, and the resulting Difference Curve, Phase H is distinct from Phase G. In terms of these graphs, however, H is less strongly represented than Phases E, F or G, say. Indeed, if consideration is restricted to Classes T and W, it is represented only by

52°	0°	160	Q 580	4800 ± 120	T
51°	4°	168	GrN 1139	4780 ± 80	T
51°	4°	174	GrN 1144	4650 ± 70	W
54°	8°	116	KI 95	4620 ± 50	T
51°	4°	173	GrN 191	4590 ± 150	W

Furthermore, as noted above, although the Class P and S curves reflect an increase of marine influence at the time of Phase G and Phase H, they do not indicate any intervening regression. Because of the imprecise nature of the evidence included in these Classes, this is not decisive, but it suggests that the distinctness of Phase H from Phase G should be investigated particularly closely.

That these Phases were in fact separate events is demonstrated by the evidence of 16 widely distributed sites. Nine of these are represented by two dates each within the period of interest. These are sites 25, 34, 132, 143, 144, 160, 166, 168, 188. The remainder show sequences fixed by one determination in this period and others earlier or later. These are sites 112, 113, 116, 127, 150, 167, 190. At each of these sites, the stratigraphy suggests strongly that H represented a distinct phase of marine deposition. It is also notable that only two Class R dates coincide with the peak of Phase H. These are GrN 238 4765 ± 130 and Hv 292 4710 ± 90.

From about 4600 onwards, however, evidence of regression is completely dominant. It builds up until the nominal end of Phase H at 4300 B.P.

The determinations involved are:

51°	4°	166	GrN 348	4620 ± 180	R
51°	3°	180	GrN 1566	4620 ± 80	R
54°	2°		Q 88	4616 ± 112	R
59°	18°		St 616	4600 ± 120	I
51°	3°		GrN 329	4595 ± 150	R
61°	17°	15	St 711	4585 ± 90	I
51°	3°		GrN 414	4555 ± 130	R
53°	7°	138	Hv 248	4530 ± 80	R
51°	3°	179	GrN 385	4500 ± 120	R
69°	15°		T 266	4500 ± 150	R
60°	17°	30	U 2109	4470 ± 110	I
51°	4°	168	GrN 1142	4400 ± 130	R
54°	9°	109	Hv 631	4395 ± 100	R
52°	0°	161	Q 263	4390 ± 120	R
51°	4°	164	GrN 1098	4380 ± 75	R
51°	4°	170	GrN 1041	4370 ± 60	R
51°	4°	175	GrN 2921	4360 ± 130	R
60°	17°	30	U 639	4350 ± 80	I
52°	14°	153	GrN 633	4350 ± 130	R
51°	3°		GrN 332	4340 ± 170	R
59°	16°		St 496	4325 ± 110	I
52°	4°	156	GrN 1135	4320 ± 65	R
61°	17°	16	St 770	4315 ± 145	I
53°	6°	144	GrN 499	4300 ± 130	R

(vii) The period between transgressive Phases H and I

This period will be considered as a unit because of the possibility, indicated in (ii) above, of an additional unlabelled transgressive phase at this time. The main evidence referring to Phase I, per se, will be examined in Part III (Chapter 10), along with that appertaining to Phase J.

Before discussing the possible additional transgressive phase, it is necessary to reiterate that evidence of decreasing marine influence heavily outweighs evidence of transgression for almost 800 years after 4600 B.P. It is against this background of a general marine regression that the possibility of a minor transgression must be viewed. The evidence of regression in this period is certainly substantial. The determinations showing it building up between 4600 and 4300 B.P. have already been listed, under Phase H. After 4300, the evidence of regression continues thus:

51°	3°	176	GrN 1036	4295 ± 55	R
51°	3°	176	GrN 1035	4280 ± 55	R
51°	3°	176	GrN 1039	4280 ± 55	R
53°	6°	143	GrN 656	4260 ± 90	R
51°	14°	167	GrN 256	4250 ± 150	R
52°	4°		GrN 1623	4240 ± 50	R
59°	18°		St 807	4235 ± 110	I
60°	17°	26	U 452	4230 ± 90	I
51°	4°	175	GrN 294	4225 ± 190	R
51°	3°		GrN 1136	4195 ± 55	R
52°	0°		Q 544	4145 ± 110	M
53°	8°		Hv 287	4170 ± 75	R
54°	9°	111	Hv 179	4160 ± 125	R
52°	4°	151	GrN 151	4140 ± 70	R

53° 8°		Hv 342	4140 ± 90	R
51° 4°	165	GrN 315	4130 ± 130	R
56° 16°	80	Lu 187	4110 ± 100	I
47° 2° W	202	Sa 42	4100 ± 300	M
71° 25°		T 244	4100 ± 150	R
71° 24°		T 186	4100 ± 100	R
51° 4°		GrN 310	4085 ± 150	R
52° 0°	161	Q 264	4085 ± 110	R
53° 7°	135	Hv 545	4070 ± 120	R
51° 3°	177	GrN 421	4060 ± 120	R
61° 17°	14	St 713	4050 ± 65	I
53° 8°		Hv 284	4030 ± 80	R
53° 7°	135	Hv 544	4010 ± 90	R
59° 17°		U 434	4000 ± 100	I
59° 17°		St 1279	3995 ± 120	I
60° 17°	26	U 471	3990 ± 90	I
51° 4°		GrN 202	3985 ± 170	R
61° 17°	16	St 716	3970 ± 100	I
52° 4°	154	GrN 460	3965 ± 110	R
53° 8°	128	Hv 164	3960 ± 200	R
53° 7°	138	Hv 246	3935 ± 75	R
52° 0°	162	Q 490	3915 ± 120	R
60° 17°	30	U 664	3910 ± 100	I
52° 0°	162	Q 489	3905 ± 120	R
53° 8°	133	Hv 289	3880 ± 85	R
53° 8°		Hv 281	3870 ± 80	R
53° 8°		Hv 341	3850 ± 80	R

51° 4° <u>169</u> GrN 286 3820 ± 180 R

59° 18° St 969 3750 ± 100 I

As Fig. 9.3 shows, the maximum accumulation of the evidence of regression listed here and under <u>H</u> occurs at 4300 B.P. The amount of Class R I evidence then decreases abruptly until about 4100 B.P., when the R I curve levels out in a distinct step for about a century before resuming the rapid decline which coincides with the onset of transgression Phase <u>I</u>. It was suggested in (ii) above, that viewed in conjunction with the pattern of the graphs of the T W and P classes of evidence which show maxima between 4300 and 4200 B.P., this was consistent with a short transgression terminated by a brief reassertion of conditions of regression before Phase <u>I</u> set in.

The stratigraphy at at least 20 widely distributed multiple-date sites appears to be consistence with the existence of a brief alternation in the trend of marine influence at this time. Nine of these sites show stratigraphy that is compatible with an event of this kind, but do not yield definitive proof. These are sites <u>16</u> (61° 17°), <u>65</u> (57° 18°), <u>109</u> (54° 9°), <u>111</u> (54° 9°), <u>119</u> (54° 8°), <u>135</u> (53° 7°), <u>140</u> (53° 7°), <u>140</u> (53° 7°), <u>158</u> (52° 0°), <u>174</u> (51° 4°), <u>175</u> (51° 4°). The remaining eleven sites, however, all give specific support. These are <u>113</u> (54° 9°), <u>127</u> (53° 9°), <u>130</u> (53° 8°), <u>132</u> (53° 8°), <u>138</u> (53° 7°), <u>146</u> (53° 5°), <u>151</u> (52° 4°), <u>159</u> (52° 0°), <u>161</u> (52° 0°), <u>202</u> (47° 2°<u>W</u>).

In the cases of sites <u>130</u>, <u>132</u>, <u>138</u>, <u>146</u>, <u>151</u> and <u>202</u> the evidence is particularly clear, with pairs of dates defining both the beginning and the end of a distinct episode of transgression in this period.

Some single-date sites also appear to offer confirmation. At Barnamossen in west Sweden (56° 12°), for instance, a sample reflecting a groundwater rise over an earlier weathered blue marine clay gave a date of St 2166, 4210 ± 130, while in Huntingdonshire (52° 0°), Q 474, 4345 ± 110 dates pine cones from a pine wood killed by transgression.

In the latter case shells of Cardium edule (cockle) are in positions of growth on the peat surface. The overlying deposit is Fen Clay. Q 580, Q 499 (both site 160), Q 31, Q 589 (both single-date sites, also 52° 0°) together with Q 130 (site 159) all refer to the stage immediately preceding this deposit, and appear to confirm Q 474. On the other hand Q 263, 4390 ± 120 (site 161 52° 0°) appears to contradict Q 474, since Q 263 was intended to date the end of the Clay deposition. It is however itself in conflict with its duplicate sample, Q 264, 4085 ± 110 also from site 161 from peat overlying the Fen Clay. Furthermore, Q 474 and Q 264 are supported by a date from Ely which places the end of the Fen Clay phase at Q 544, 4195 ± 110 (52° 0°).

In Finnmark, Marthinussen distinguishes a minor shoreline, which he designates N4, from his Tapes IV line. Tapes IV coincides with Phase H, and N4 appears to agree closely with the minor transgression phase under investigation here (T 244 and T 186, see below).

When the evidence of the sites showing a distinct transgression in this period is considered in detail, it would appear that the most reliable dates for the onset of the transgression are:

53°	7°	138	Hv 247	4375 ± 90	T
53°	7°	140	GrN 1088	4350 ± 75	W
52°	0°		Q 474	4345 ± 110	M
51°	4°	174	GrN 1146	4270 ± 55	W
54°	8°	119	KI 232	4240 ± 60	P
54°	8°	119	KI 233	4230 ± 75	P
56°	12°		St 2166	4210 ± 130	M

Similarly, the most reliable indications of its termination are

52°	0°		Q 544	4195 ± 110	M

47° 2°W	202	Sa 42	4100 ± 300	M
71° 25°		T 244	4100 ± 150	R
71° 24°		T 186	4100 ± 100	R
53° 5°	146	GrN 610	4090 ± 120	M
52° 0°	161	Q 264	4085 ± 110	R

It will be recalled that the conclusions from the present chapter will be summarised in the opening section of Chapter 11.

Figure 9.1 Identification of Phases between 7000 and 3000 B.P. in terms of the Difference Curve.

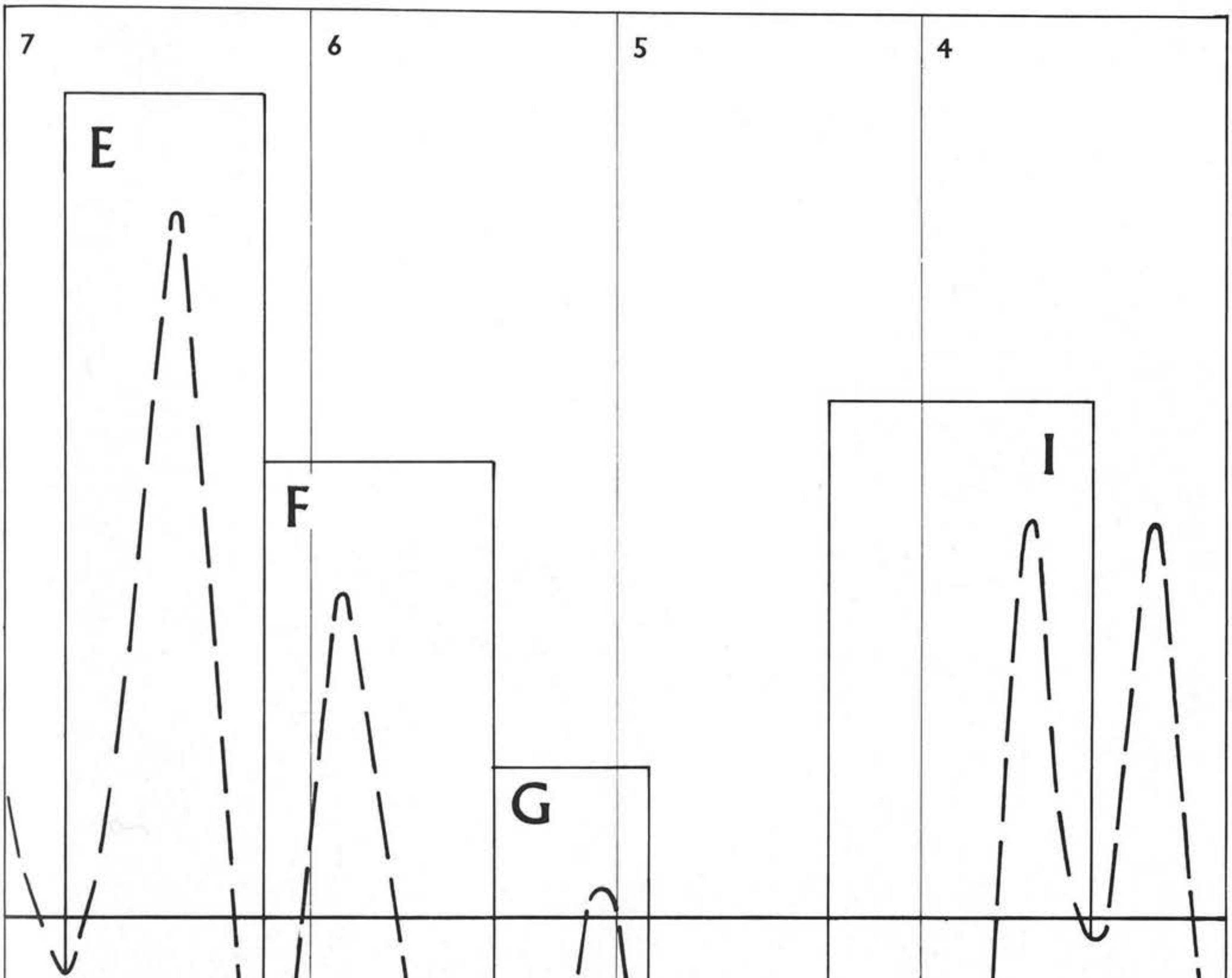
7
6
5
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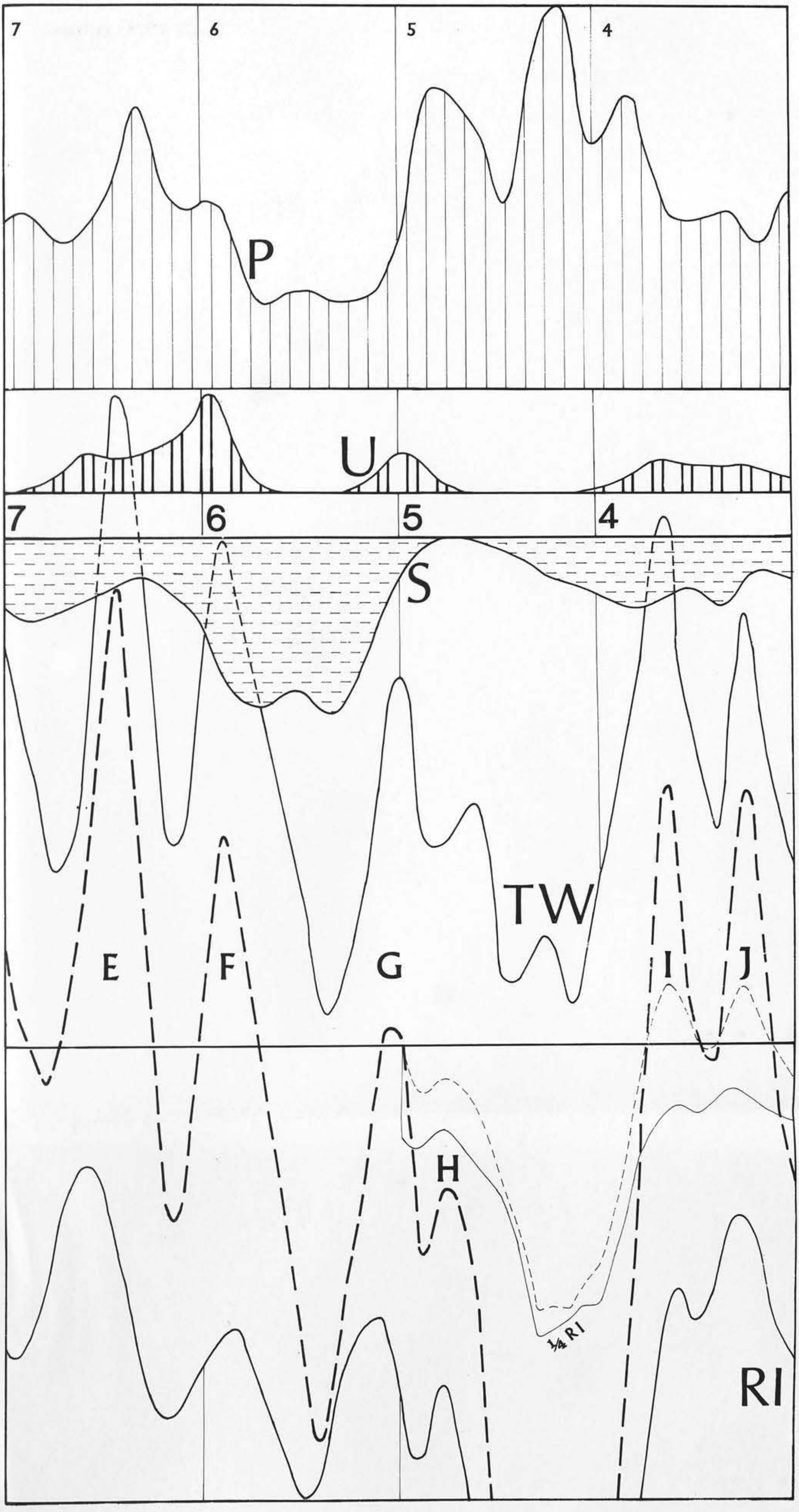
Figure 9.2 All Classes of C^{14} evidence from 7000 to 3000 B.P., with part of the RI and Difference Curves at $\frac{1}{4}$ of the general ordinate scale.



7
6
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S
TW
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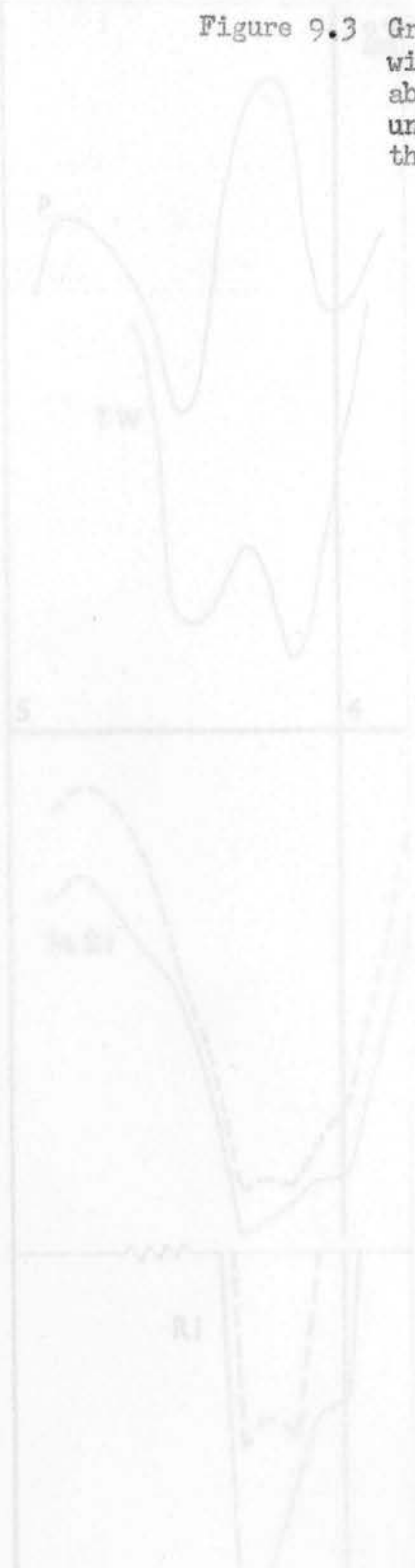
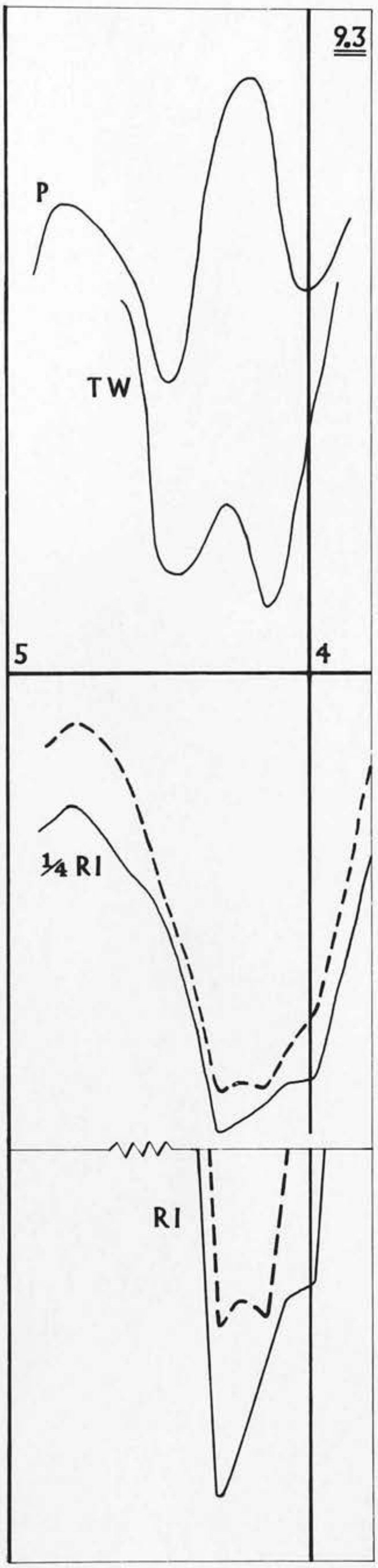
1/2 RI
RI

Figure 9.3 Graphs of the 4300 B.P. episode, with the ratio between the abscissa and ordinate scales uniform with that employed throughout the main graphs.




9.3
P
TW
5
4
¼ R1
R1

Chapter 10

The overall pattern of the radiocarbon dated evidence from the western seaboard of Europe, excluding Scotland

Part III : 4,000 B.P. to the present

Introduction

In section (i) of this Part, the phases of transgression represented by the Class T W R I graphs are identified and their general characteristics are described. Then in (ii) the relationships between these classes and the overall patterns of the other main categories of evidence (Classes S, P, and U) are investigated. In (iii) the date sequences at multiple-date sites are identified, and radiocarbon determinations of limited value in the present context are indicated for the whole of the period covered in this Part. Phases I, J, K, L, M and N are then examined individually in (iv) to (ix) respectively, while the remainder of the period up to the present day is dealt with in (x). As in the cases of Parts I and II, the results from this Part are summarised at the start of Chapter 11.

(i) The graphs used in formulating the initial hypothesis suggest eight phases of marine transgression after 4000 B.P. These are identified in terms of the Difference Curve in Fig.10.1, and illustrated more fully in Fig. 10.2.

It has already been remarked that whereas the first four millennia of the Holocene were characterised by a definite dominance of transgression evidence and this was succeeded until 4000 B.P. by an equally marked swing towards evidence of regression, a more even balance exists in the period considered in the present Chapter.

Overall, this is so. However, the statement must now be qualified by noting that although imbalances of the extreme sort apparent in the previous periods are absent, the balance also shifts somewhat after 4000 B.P. Thus from

4000 to 3000 B.P. the Difference Curve tends to favour transgression, then for the next millenium regression. After a final swing towards transgression between 2000 and 1500 B.P., it oscillates about the zero line until the present day.

Despite these variations, the maxima on the Difference Curve of all transgression Phases identified in this Part lie above the abscissa (unlike the proposed Phases A and H), and all the intervening minima (except that between M and N) lie below it, unlike those between B and C, C and D, E and F in the earlier Holocene so, overall, the generalisation seems valid.

The higher frequency of oscillations in the Mean and Difference curves after 4000 B.P. has also been mentioned in discussing the earlier parts of the Holocene. It will be seen that according to the graphs the duration of these later Phases is as follows:-

Phase	Starts	Peak	Ends
I	(see note)	3650	3450
J	3450	3250	2900
K	2900	2700	2500
L	2500	2350	2150
M	2150	1900	1800
N	1800	1700	1400
O	1400	1150	850
P	850	450	350

(Note: The possibility of a minor unlabelled Phase between H and I opens the question of whether Phase I should be regarded as beginning at 4300 or 4150 B.P. This is discussed further in Chapter 11.)

The average difference between the peak and the end of a Phase is less than 220 years, and in two cases (M and P) it is as little as 100 years. 63% of the radiocarbon determinations available for the four millennia under discussion here have standard deviations equal to or greater than ± 100 years, while almost half (over 48%) have two standard deviation limits equal to or greater than ± 220 years.

It will be recalled that in the earlier parts of the Holocene some peaks of transgression evidence were separated by the better part of a millennium, while the areas of uncertainty surrounding the radiocarbon determinations were on average only slightly bigger than those in the period now under consideration (Chapter 5). The geological implications of this change in the nature of the evidence will be discussed at a later stage. Without prejudicing that discussion, it may be noted here that one result of the change is a deterioration of the "signal to noise ratio" of the evidence, compared with that prevailing in most of the earlier periods of the Holocene. Thus, whereas transgression phases were often previously separated by periods of some centuries (indeed, up to half a millennium) in which regression evidence was completely dominant, in the period of rapid oscillations after 4000 B.P. the alternation of the different classes of evidence is generally less clearly defined, although as the graphs show it certainly still occurs.

(ii) Altogether, a total of 274 radiocarbon dates are available for the period between 4000 B.P. and the present day. Only 20 of these fall in the most recent thousand years. Since this millennium lies fully within the historical period, and hence outwith the scope of the archaeological material considered in the present study, these dates and Phase P, proposed on the basis of their evidence, will not be discussed in detail.

Fig. 10.2 shows the overall patterns of the evidence of Classes T W, R I, S, U and P. The presentation is handled in the same way as in the equivalent diagram in Part II. (Fig. 9.2)

The relationship of the Class P curve to the T and Difference curves is also shown in Fig. 10.3 (to avoid visual confusion, in this case the Difference Curve is shown at one quarter vertical scale).

In Chapter 8 it was noted that the Class P curve tended to match the T W curve most closely in the latter part of the Holocene, when the alternating peaks of transgression and regression evidence were in general smaller and more frequent than in earlier phases.

This statement may now be refined. It will be noticed that turning points of the Class P curve (both maxima and minima) tend in general to fall slightly earlier than the corresponding turning points of the T W curve. The discrepancy is small but fairly consistent in amount. In Fig. 10.3, the Class P curve has accordingly been displaced to the right relative to the T and Difference curves by half a century.

It will be seen that the majority of the turning points now match very closely. It will also be noticed that in the cases of the largest remaining discrepancies (Phases I and M) the Class P turning points still precede those of the T curve.

This pattern, with peaks of Class P evidence immediately preceding the maxima of the transgression contacts (Class T), appears to bear out the suggestion made in Chapter 8 that in the later Holocene most thin intermarine peat beds reflect rises in watertable immediately preceding the full transgression of their sites.

The curves of the Class U and S evidence do not correspond as completely as the P curve to the pattern of the T W R I data. Some degree of relationship is nevertheless apparent.

The Class U curve shows only three maxima, all prior to 2000 B.P. The first two of these are not well marked, being represented by only one date each, but it is notable that these coincide very closely with the maxima of transgression Phases I and J (ca. 3650 and 3250 B.P. respectively). The dates are from the submerged forest beds at Mounts Bay (50° 5°W) and St. Aubin-sur-Mer (49° 0°W) and are

50° 5°	BM 29	3656 $\pm$ 150	U
49° 0°W	Sa 223	3220 $\pm$ 200	U

The other Class U peak which coincides particularly closely with a peak of the T W curve (Phase G) also consists of only one date. The larger U-maxima all tend to slightly precede the corresponding T W maxima. This may be the case with the final U-maximum, though the rapidity of the alternation of T W and R I peaks makes it difficult to be sure. This U-peak falls at 2130 B.P. in the regression phase between L and M, but is twice as near to the M peak of the T W curve, which it precedes, as it is to the N peak. The majority of the evidence included in this peak in fact coincides rather more closely with Phase M than the graph suggests, because this peak envelops Sa 190 (inset on the diagram) which coincides exactly with the maximum of Phase N.

A peak of shell evidence, believed from field observation to represent a transgressive phase centred about 2350 B.P. in fact agrees well with the peak of transgression in Phase L, while a similar peak in the most recent millennium may in part correspond to Phase P. In the latter case, however, some of the Class S evidence plotted above the abscissa refers only to very recent shells found above the reach of present wave action and its interpretation is by no means secure. By analogy with the pattern evident in the earlier parts of the Holocene, the general S curve, below the abscissa, may also reflect Phases I, J, O and P, but little reliance may be put on this.

Overall, then, it would seem that Phase I is represented by maxima of the T W, U and P curves and corresponding minima of the R I (and perhaps general S) curves. The same is true of Phase J. The Class U and S curves show no indication of Phase K, but this is registered in the T W, R I and P curves, as is Phase L, which is also registered by a peak of shell evidence that indicates transgression. Phase M is represented by the T W and R I curves, and apparently also by the immediately preceding peaks of class U and P evidence. Phase N is registered clearly by the T W, R I and P curves. The general S curve appears to disagree but little weight can be put on this since the circumstances in which the shells involved (NPL 25; T 181 bis) were found do not define regression conditions specifically, and indeed the contrary may be the case at either of these sites. Phase O appears to be represented only by the T W and R I curves, although the peak of the general S curve at the beginning of the present millennium may possibly reflect the O/P maximum of the R I curve.

(iii) Multiple-date sites represented between 4000 B.P. and the present.

69° 16°	4	T 380, T 353
61° 17°	16	St 770, St 716
	17	St 775, St 714
	18	St 747, St 708
	19	St 772, St 717
	20	St 771, St 709, St 776
60° 17°	26	U 452, U 471
	27	U 472, U 642, U 2024
	28	U 225, U 2025, U 704
	30	U 639, U 664
59° 16°	35	U 436, U 430, U 435
	36	U 432, U 433, U 1005
59° 14°	39	U 513, U 2021
59° 10°	43	T 89B, T 89A

	44	T 91A, T 91B
58° 17°	46	U 700, U 702
57° 18°	66	St 1722, St 1624
56° 15°	80	Lu 187, Lu 147
	81	St 1003, St 811
56° 12°	86	St 2214, St 1815
	92	St 2203, St 2176
	94	St 2336, St 1195
54° 9°	110	Hv 764, Hv 766
	111	Hv 179, Hv 176
	112	Hv 229, Hv 231
	113	Hv 219, Hv 220
	114	Hv 241, Hv 242, Hv 243
54° 8°	115	K 796, K 795, K 797
	116	KI 95, KI 100, KI 200, KI 201
	117	KI 203, KI 98
	118	KI 97, KI 202, KI 99, KI 204
54° 5°W	120	LJ 903, LJ 908, LJ 907, LJ 906, LJ 905
	121	Q 770, Q 633, Q 635
	122	I 1198, I 1199
53° 10°	125	Hv 662, Hv 561
	126	Hv 309, Hv 308
53° 9°	127	Hv 27, Hv 28, Hv 26
53° 8°	128	Hv 164, Hv 163
	129	Hv 53, Hv 53a, Hv 52
	130	Hv 100, Hv 99, Hv 98, Hv 97
	132	Hv 109, Hv 108, Hv 107, Hv 106, Hv 105

	133	Hv 289, Hv 290
	134	Hv 292, Hv 291
53° 7°	135	Hv 544, Hv 542, Hv 543
	136	Hv 890, Hv 889, Hv 891
	137	Hv 38, Hv 37
	138	Hv 247, Hv 246, Hv 245
	139	Hv 41, Hv 40, Hv 39
	140	GrN 1088, GrN 1090, GrN 1089
53° 6°	141	GrN 4151, GrN 4222, GrN 4110, GrN 4221
	142	GrN 637, GrN 619
	144	GrN 499, GrN 602
53° 5°	146	GrN 610, GrN 609, GrN 617
53° 3°W	147	Q 620, Q 620 dup.
0°W	148	Q 77, Q 78
	149	Q 685, Q 686, Q 688, Q 687, Q 844
52° 4°	154	GrN 476, GrN 460, GrN 463
52° 4°W	157	Q 382, Q 712
52° 0°	162	Q 490, Q 489, Q 805, Q 807, Q 806, Q 550, Q 549
	163	Q 829, Q 819, Q 820, Q 823
51° 4°	164	GrN 1098, GrN 1096, GrN 1095, GrN 1094, GrN 1093
	165	GrN 315, GrN 308
	166	GrN 1113, GrN 346
	167	GrN 256, GrN 220
	169	GrN 286, GrN 296
	173	GrN 191, GrN 192
	174	GrN 1146, GrN 1147, GrN 1148
51° 3°W	184	Q 274, Q 265

51° 2° <u>187</u> Lv 91, Lv 86, Lv 90, Lv 85

51° 0° <u>190</u> Q 792, Q 792 dup., Q 793, Q 793 dup.

50° 1° <u>192</u> <u>Q 835</u>, Q 831, Q 832

50° 0° <u>196</u> NPL 92, NPL 91

49° 0° <u>197</u> Sa 71, Sa 68

48° 1°<u>W</u> <u>199</u> <u>Gif 390</u>, Gif 391

48° 4°<u>W</u> <u>200</u> <u>Gsy 47B</u>, Gif 278, Gif 47c, Gif 159, Gif 160.

47° 2°<u>W</u> <u>201</u> Gif 193A, Gif 193B, Gif 193C

<u>202</u> <u>Sa 35</u>, Sa 40, Sa 43

163 of the 274 radiocarbon dates available for the period after 4000 B.P. come from sites represented by series of dates rather than individual determinations. In all, 76 of these multiple-date sites are involved.

As indicated in Chapter 5, perturbations in the radiocarbon input curve are only evident in the most recent millennium and prior to 3750 B.P. It was shown in Chapter 9 that the perturbations in the group preceding 3750 were small, and that they were apparently not of importance in the present context. Certainly no inversions of date order relative to stratigraphy coincided with that group. Indeed the only inversion after 4000 B.P. occurs at site <u>162</u>, Saddlebow in the Fenland (52° 0°), and as noted in Chapter 5 the cause of this is clearly contamination since the whole monolith from which the samples were drawn is penetrated by vertical roots of Phragmites.

Suggestions of contamination also make it desirable to disregard sites <u>32</u>, <u>35</u>, <u>36</u>, <u>43</u>, <u>46</u>, <u>86</u>, <u>163</u> and <u>197</u>, together with some individual dates from the series at six other multiple-date sites (<u>27</u>, U 2024; <u>28</u>, U 2025 and U 704; <u>30</u>, U 664; <u>39</u>, U 2021; <u>141</u>, GrN 4151 and GrN 4222). This is also so of some single-date sites (Gsy 59; St 945, 2211, 2433, 2728i/ii, 2764; U 221, 440).

The value of some other dates is limited in the present context because of the way in which they have been published (St 642, 2158, 2173, 2176) or because their interpretation in terms of transgression or regression is unclear in other respects. Multiple-date sites in the latter category are 133 (Hv 290 only), 134, 187, 200, and 201, and single-date sites are represented by: Gif 194 and 762; I 2556; Q 545 and 546; St 2340; U 44. Shell dates of this kind are: 146 GrN 617; 141 GrN 4221; Hv 300; K 867; NPL 25; Sa 224; St 1167; T 122; T 181 bis; 4 T 353; U 607.

(iv) Phase I

According to the Difference Curve, Phase I reaches its maximum about 3650 and ends at circa 3450 B.P. The possibility of an additional unlabelled phase between H and I but prior to 4000 B.P. was discussed in the previous chapter. Accordingly, only the period after 4000 B.P. will be discussed here. Altogether, almost seventy radiocarbon dates are available for the period between 4000 and 3450 B.P.

When the imperfect evidence listed in (iii) above is omitted, 31 multiple-date sites are represented within this period, site 116 by three dates and sites 17, 75, 116, 132, 147, 174, and 190 by two dates each. The remaining sites, with sequences fixed by one date within this period and others earlier or later are 6, 16, 26, 30, 66, 110, 112, 113, 114, 117, 120, 121, 128, 130, 133, 136, 138, 146, 149, 154, 164, 193, 194 and 199.

At none of these sites except 132 is there any definite indication that more than one episode of transgression occurred during this period. Site 132 shows a complex sequence of thin peat layers interbedded with marine silty clay. Two of these are dated 3940 ± 110 and 3710 ± 140 (Hv 108, Hv 107 respectively). This site also shows more than one peat layer in Phase H (see Chapter 9, above) and in the period between Phase J and Phase K (see below). Since all these cases appear to be anomalous in terms of the stratigraphy at the great majority of the other multiple-date sites it seems safe to conclude that the site 132 peat beds reflect local rather than eustatic factors.

It was remarked in (ii) above, that transgressions during Phase I appeared to be evident in Classes P and U as well as T and W.

Some evidence of transgression is apparent from 4000 B.P. onwards, but until ca. 3850 this is more than outweighed by evidence of regression. It is indeed restricted to three Class T dates (112 Hv 229, 4000 ± 80; 149 Q 685, 3943 ± 100; 66 St 1722, 3930 ± 105) and one of Class W (174 GrN 1147, 3900 ± 70). Three Class P dates probably also signify transgression (190 Q 792 i, 3940 ± 110; 132 Hv 108, 3940 ± 110; 130 Hv 99, 3900 ± 120), though site 132 has already been noted to be exceptional in its stratigraphy.

Regression is however apparent in the following cases:

59°	17°		U 434	4000 ± 100	I
59°	17°		St 1279	3995 ± 120	I
60°	17°	26	U 471	3990 ± 90	I
51°	4°		GrN 202	3985 ± 170	R
61°	17°	16	St 716	3970 ± 100	I
52°	4°	154	GrN 460	3965 ± 110	R
53°	8°	128	Hv 164	3960 ± 200	R
53°	7°	138	Hv 246	3935 ± 75	R
53°	8°	133	Hv 289	3880 ± 85	R
53°	8°		Hv 281	3870 ± 80	R
53°	8°		Hv 341	3850 ± 80	R
51°	4°	169	GrN 286	3820 ± 180	R

From 3820 on for 250 years, no more dates signifying decreases of marine influence occur, except for St 969 and St 775, (3750 ± 100 and 3670 ± 150) both dating the isolation of bogs from the sea in central Sweden, where isostatic uplift was particularly marked. Otherwise, this period is entirely dominated by evidence of marine transgression:

51°	4°	164	GrN 1096	3840 ± 75	T
54°	8°	116	KI 100	3820 ± 60	T
53°	7°	136	Hv 890	3805 ± 80	P
53°	5°	146	GrN 609	3750 ± 120	T
50°	1°	194	Gif 399	3750 ± 200	P
53°	6°W		D 31	3730 ± 130	P
50°	1°	193	Gif 397	3720 ± 200	P
53°	8°		Hv 158	3710 ± 140	P
53°	8°	132	Hv 107	3710 ± 140	P
53°	3°W	147	Q 620 i	3695 ± 110	T
54°	8°	116	KI 200	3690 ± 50	T
50°	1°	192	Q 831	3689 ± 120	M
53°	3°W	147	Q 620 ii	3680 ± 110	T
54°	9°		Hv 215	3670 ± 100	T
50°	5°W		BM 29	3656 ± 150	U
54°	8°	117	KI 203	3650 ± 40	T
54°	8°	115	K 796	3650 ± 120	M
54°	9°	110	Hv 764	3640 ± 85	T
54°	9°	114	Hv 241	3620 ± 115	T

Two other sites buried by marine transgression appear to date from this phase. The occupation layer beneath beach gravel at Ringniell Quay in Ireland yielded

54°	5°W	121	Q 633	3680 ± 120	M

while charcoal from a buried paleosol on the Cumberland coast yielded

54°	3°W		Y 2387	3630 ± 160	M

After 3600 B.P., dates signifying regression briefly become dominant again before the onset of Phase J. There is some indication that this regression is registered by the general curve of Class S evidence (as shown in ii, above), but it is not considered that the inflexion of the graph is decisive enough to justify distinguishing specific shell sites here in the absence of reliable field evidence that the shell beds concerned indeed represent conditions of regression.

The clearest evidence of regression is:

68°	13°	6	U 99	3750 ± 90	R
50°	1°	192	Q 832	3563 ± 96	M
54°	9°	113	Hv 220	3500 ± 80	R
61°	17°	17	St 714	3480 ± 80	I
52°	4°		GrN 1569	3470 ± 60	R

The last of these dates overlaps with the first of the dates indicating the transgression of Phase J by 40 years, but this is less than one standard deviation of the determinations involved.

The small number of the dates clearly indicating regression between the transgressions of Phases I and J, is however of geological interest, in that it raises the question of whether it is valid to regard these transgressions as separate events. On balance, it would seem that this is so. The T W, R I and P graphs support the distinction clearly (as do Classes S and U, to a lesser extent). Although the dates indicating regression are small in number they form a well defined grouping in time (all falling within a single century), yet are widely distributed in geographical terms (from 50° to 68° North by 1° to 17° East). Finally, the stratigraphy of at least eight widely distributed sites appear to offer additional stratigraphic support for the distinction. These are sites 118, 120, 149, 183, 184, and especially 117 and 140.

(v) Phase J

According to the Difference Curve, Phase J begins about 3450, reaches its maximum around 3250 and ends at 2900 B.P. Altogether, almost sixty radiocarbon dates are available.

When the imperfect evidence listed in (iii) above is omitted, 21 multiple-date sites are represented in this period, three by two dates each (sites 118, 139, 140) and the remainder by sequences fixed by one date within this period and others earlier and later (4, 18, 80, 111, 114, 117, 120, 122, 127, 132, 133, 135, 138, 146, 149, 157, 164, 184). As in the case of Phase I, only at the exceptional site 132 does the stratigraphy suggest more than one episode of transgression in this phase.

It will be recalled from (ii) above, that transgression during Phase J appears to be represented not only by Class T and W evidence, but by Classes P and U. As noted in (iv), the last evidence of regression between I and J fell at 3470 ± 60 (GrN 1569). The earliest Phase J evidence of transgression overlaps with this by some 40 years, but this is less than one standard deviation of the dates involved.

For some 450 years after 3500 B.P. evidence of transgression is dominant:

54° 8°	116	KI 201	3510 ± 55	T
52° 5°		GrN 378	3505 ± 120	T
53° 8°		Hv 54	3500 ± 110	T
51° 4°	174	GrN 1148	3480 ± 50	W
51° 3°W	183	NPL 146	3460 ± 90	M
49° 2°W		Sa 202	3460 ± 200	M
51° 3°W	184	Q 265	3402 ± 108	T
54° 8°	115	K 795	3400 ± 120	W

54° 5°W	120	LJ 908	3380 ± 180	T
53° 7°	140	GrN 1090	3350 ± 60	W
50° 0°		NPL 24	3340 ± 92	P
52° 5°		GrN 377	3315 ± 90	P
52° 0°		Q 547	3305 ± 120	T
54° 8°	118	KI 97	3300 ± 50	T
54° 8°	117	KI 98	3255 ± 40	T
53° 7°	138	Hv 245	3220 ± 60	T
49° 0°W		Sa 223	3220 ± 200	U
53° 8°	132	Hv 106	3200 ± 140	P
49° 2°W		Q 736	3180 ± 110	T
53° 7°	135	Hv 542	3160 ± 70	T

In addition to these dates, two others in this period show water tables rising apparently under marine influence in bogs very close to sea level. These are

51° 4°	169	GrN 296	3325 ± 120	M
53° 8°		Hv 286	3290 ± 60	M

Only three dates suggesting decreases of marine influence fall before 3150 B.P. in Phase J. Two of these refer to the isolation of lakes from the sea: site 18 St 747, 3270 ± 105; site 80 3370 ± 100. In the latter case the sample is of lacustrine detritus gyttja from just above isolation level. Since the corresponding sample there from the marine sediment just below that level yielded a date 740 years older, little weight can be put on this particular date. The third exceptional date was from site 149, Q 686, 3340 ± 110, and referred to saltmarsh peat from base of an upper peat at the contact with an underlying Phragmites clay. This would certainly appear to represent a regression contact. The fact that only sites 18 and 149 show decreases of marine influence at this period suggests however that if these dates are correct they represent only local changes.

From 3150 B.P. onwards regression evidence is dominant:

51° 4°		GrN 159	3145 ± 150	R
54° 5°W	122	I 1199	3125 ± 150	R
53° 0°		Q 715	3120 ± 105	M
54° 9°	114	Hv 242	3110 ± 80	R
53° 7°	139	Hv 40	3075 ± 100	M
69° 16°		T 351	3070 ± 100	R
58° 7°	54	T 281	3050 ± 100	R
52° 4°		GrN 1150	3000 ± 65	R
56° 14°		St 1749	2990 ± 120	I
52° 4°W	157	Q 712	2900 ± 110	R
51° 4°	164	GrN 1095	2900 ± 60	R

There are only two possible exceptions in this period of regression. One is at site 118 where KI 202, 3070 ± 60 dates peat overlaid by marine clay. The other is at site 158, Q 531, 3065 ± 110. There an unconformity may be present where an apparent transgression contact was sampled. The date is certainly earlier than anticipated by Godwin and West, and they note an abrupt transition between peat and overlying silt (Radiocarbon III 1961, p.69).

(vi) Phase K

According to the Difference Curve, Phase K begins about 2900 B.P., reaches its maximum around 2700 and ends at 2500 B.P. Altogether, almost forty radiocarbon dates are available.

When the imperfect evidence listed in (iii) above is omitted, 17 multiple-date sites are represented in this period, five by two dates each (sites 129, 130, 141, 148, 164) and the remainder by one date in this period and others earlier or later (18, 111, 120, 127, 132, 135, 144, 149, 154, 157, 173, 177). At none of these sites does there appear to be any clear suggestion of more than one episode of transgression within the period of this Phase.

It was noted in (ii) above, that although transgression during K is well represented, by a maximum of the T W curve and corresponding minimum of the R I curve, it is not evident in Class U or transgressive S evidence. It will be recalled that although the Class P curve showed a maximum corresponding closely to that of the T W curve, it also showed an earlier maximum between Phases J and K. This latter peak clearly does not conform to the generalisation that Class P dates in the later Holocene tend to correspond to the onset of transgressions. The separation between these two Class P peaks is not sufficiently distinct on the graphs to allow a reliable separation of those Class P determinations which conform to the generalisation from those which do not. Scrutiny of the individual reports of the stratigraphy of the samples concerned proved of only limited value, and rather than make a partly arbitrary decision, it was considered sounder to rely on none of the Class P dates when considering Phase K. They are accordingly omitted here.

Despite this omission, the first part of Phase K is marked by a well defined accumulation of evidence of transgression:

53° 7°	140	GrN 1089	2950 ± 70	W
53° 7°	139	Hv 39	2930 ± 70	T
53° 6°	144	GrN 602	2875 ± 110	T
51° 4°	173	GrN 192	2830 ± 135	W
53° 0°	149	Q 844	2815 ± 100	T
53° 0°W		Q 79	2796 ± 100	M
53° 0°W	148	Q 78	2784 ± 100	M
53° 7°	135	Hv 543	2710 ± 50	T
53° 0°		BM 58	2700 ± 150	M

From then on, evidence of regression has an overall dominance. The end of the period of transgression and the beginning of the period of regression overlap, but by no more than 80 years, which is not meaningful, in view of the statistical errors involved:

69° 15°		St 926	2780 ± 65	R
47° 2°W	202	Sa 43	2770 ± 300	M
69° 16°	2	T 385	2710 ± 90	R
54° 5°W	120	LJ 907	2710 ± 150	R
70° 25°		T 243	2450 ± 100	R
59° 18°		St 1184	2660 ± 110	I
50° 0°W		Q 690	2620 ± 110	R
60° 17°	32	U 574	2610 ± 70	I
61° 17°	18	St 708	2535 ± 110	I
52° 4°	154	GrN 463	2525 ± 110	M
53° 10°	125	Hv 561	2480 ± 60	R
53° 0°		Q 81	2455 ± 110	R
70° 25°		T 243	2450 ± 100	R

Although the period between 2700 and 2450 B.P. shows an overall dominance of evidence of regression, some transgression contacts do occur. One of these, at site 164, Lodderland in the Netherlands refers only to a thin marine clay split in a peat bed (GrN 1094, 2645 ± 65); the others, however, at two sites on the German North Sea coast (111 Hv 176, 2690 ± 105; Hv 237, 2520 ± 90) and at Llanwern in Wales (Q 691, 2660 ± 110) mark the top of peat underlying substantial beds of marine clay. Wood from the Brigg Trackway in Lincolnshire (148, Q 77, 2552 ± 120) has also been taken to date a marine transgression in the Humber/Ancholme valley.

The approximate synchronisation and widespread distribution of these sites might be taken to suggest an additional minor eustatic fluctuation within this period. The possibility cannot be completely dismissed, but it is notable that none of the 17 multiple-date sites represented in Phase K offer any stratigraphic confirmation of this.

This is so even at site 164, Lodderland. There peat growth started with the regression at the end of Phase J (2900 ± 60, GrN 1095) and persisted unbroken until it was terminated by the deposition of the clay split at 2645 ± 65, so there is again no proof of any additional fluctuation.

(vii) Phase L

According to the Difference Curve, Phase L begins about 2500, reaches its maximum at 2350 and ends around 2150 B.P. Just over thirty radiocarbon dates are available.

When the imperfect evidence listed in (iii) above is omitted, only seven multiple-date sites are represented, sites 19 and 149 by two dates each and sites 110, 121, 128, 141 and 142 by one date within this period and others earlier or later. In none of these cases is there any clear suggestion of more than one episode of transgression within this Phase.

It will be recalled from (ii) above, that transgression in Phase L is apparently represented by Class P, U and S as well as T evidence. It was shown in (vi) above, that the last clear evidence of regression between K and L fell at 2450 (T 243). Evidence of transgression is then dominant for about 250 years :

53° 8°	128	Hv 163	2440 ± 170	T
53° 8°	129	Hv 53a	2430 ± 100	P
54° 9°	110	Hv 766	2405 ± 80	T
53° 6°	142	GrN 619	2400 ± 130	T
51° 0°	190	Q 793 ii	2390 ± 110	P
47° 2°W		Sa 190	2350 ± 150	U

53° 0°W	149	Q 687	2325 ± 110	S
53° 0°W	149	Q 688	2325 ± 110	S
51° 3°	177	GrN 417	2200 ± 135	T

Only one determination (Hv 283, 2390 ± 90), an apparent regression contact, appears to be in direct conflict with this sequence. The submitters had expected this to yield the same date as Hv 340 (2090 ± 70) at a nearby section, and each of these dates seems anomalous, both in relationship to the general pattern of the evidence and in terms of other dates in their immediate neighbourhood, at Wanna on the German North Sea coast.

St 772 also coincides with the end of the period dominated by evidence of transgression but, unlike Hv 283, overlaps at one standard deviation with the succeeding period, which is dominated by evidence of regression:

61° 17°	19	St 772	2310 ± 140	I
69° 15°	2	T 350	2200 ± 100	R
59° 18°		St 1119	2160 ± 80	I
61° 17°	19	St 717	2140 ± 80	I

It will be noticed that all of these dates refer to Scandinavian sites, affected by glacio-isostatic rebound, indeed three of them reflect the isolation of bogs and lakes from the sea in the central Swedish area of high isostatic uplift. No regression contacts are represented outside Scandinavia at this time.

This raises the question of whether the transgressions of Phases L and M should be regarded as separate events. They certainly register distinctly on the T W, R I and P (though not U) graphs. Unfortunately, few multiple-date sites are represented in the period of interest, and none of these have dates on both L and M.

(viii) Phase M

According to the Difference Curve, Phase M begins about 2150 B.P., reaches its maximum at 1900 and ends about 1800 B.P. Less than 30 radiocarbon dates are available.

When the imperfect evidence listed in (iii) above is omitted, only 8 multiple-date sites are represented, none by more than one date in this period. These are sites 20, 81, 114, 118, 129, 136, 165, and 167. In none of these cases is there any clear suggestion of more than one episode of transgression within this Phase.

It will be recalled from (ii) above that transgression in Phase M is apparently represented by Class P and U evidence as well as Class T. It was shown in (vii) above that the last evidence of decreasing marine influence at the end of Phase L fell at 2140 B.P. (St 717). Evidence of transgression is then dominant for some three centuries:

53° 10°	126	Hv 308	2130 ± 70	T
49° 1°		Gif 169	2100 ± 140	U
49° 1°W		Gif 172	2130 ± 150	U
49° 1°W		Gif 171	2130 ± 150	U
50° 0°	196	NPL 91	2050 ± 90	P
53° 7°	136	Hv 889	2015 ± 75	P
56° 15°	81	St 1003	2010 ± 90	T
54° 8°	118	KI 99	2000 ± 85	T
51° 4°	167	GrN 220	1865 ± 180	T
51° 4°	165	GrN 308	1830 ± 110	T

Only one radiocarbon determination appears to be in complete disagreement with this dominance of evidence of transgression. This is Hv 340, which dates what is apparently a regression contact at 2090 ± 70. The field-workers had expected this to be the same age as Hv 283 (2390 ± 90) at a nearby section at Wanna in north west Germany. As noted in (vii) above, however, both these dates seem anomalous. It seems that little weight can thus be placed on this determination.

St 808 coincides with the end of the transgression period, but conforms to within one standard deviation with the other dates indicating regression at the end of Phase M. The clearest of these are:

59° 18°		St 808	1940 ± 100	I
54° 9°		Hv 243	1825 ± 50	R
61° 17°	20	St 771	1810 ± 140	I

As in the case of L and M this shortage of firm evidence of decreasing marine influence between Phases M and N, together with the brevity of the period separating the peaks of transgression evidence (at 1900 B.P. and 1700 B.P. respectively) raises the question of how far these phases should be regarded as representing separate events.

The T W, R I and P curves certainly all show separate turning points for M and N. Unfortunately only 3 multiple-date sites are represented by dates in both M and N. One of these (136) throws no light on the problem, but at site 20 (two stages of isolation from the sea) and site 118 (two successive transgressive contacts on the same bore) the separate identity of the phases certainly appears to be confirmed. The geographical separation of these sites (60° 17° and 54° 8°, Baltic Sweden and German North Sea coasts respectively) perhaps makes this a more valuable observation than if they had been neighbouring sites, but two cases offer little scope for confidence. The writer therefore considers that it is probably best for the moment to regard L, M and N all as subdivisions of a single phase of transgression.

(ix) Phase N

According to the Difference Curve, Phase N begins about 1800 B.P., reaches its maximum at 1700 and ends by 1400 B.P. Altogether, just under twenty radiocarbon dates are available.

When the imperfect evidence listed in (iii) above is omitted, only 8 multiple-date sites are represented, site 137 by two dates and sites 20, 118, 129, 136, 164, 166 and 199 by one date within this period, and others earlier or later. In none of these cases is there any clear indication of more than one episode of transgression within this period.

The clearest evidence of transgression is as follows. Class P evidence is again included, in conformity with the conclusion reached in (ii) above:

49° 0°W		Sa 60	1800 ± 160	M
53° 8°	129	Hv 52	1800 ± 180	P
54° 8°	118	KI 204	1710 ± 60	T
51° 4°	164	GrN 1093	1705 ± 65	T
53° 7°	137	Hv 38	1700 ± 100	P
53° 7°	136	Hv 891	1680 ± 70	P
53° 7°	137	Hv 37	1650 ± 100	T

After 1650 B.P., there is no further Class T evidence until 1180 ± 100 (K 797) in Phase O, except for GrN 346, 1415 ± 120. As will now be shown, GrN 346 falls in a period dominated by regression evidence. It is not supported by any P, U, M or transgressive S evidence, and the inflexion that it causes in the T W curve is one of those noted in Chapter 6 as exceptional in that it has no counterpart in the R I curve. It therefore seems reasonable to regard it as reflecting purely local factors or perhaps a dating error.

The clearest evidence of regression in this period between N and O is as follows:

53° 10°	Hv 559	1480 ± 50	M
52° 0°	Q 713	1464 ± 154	M

52° 4°		GrN 631	1460 ± 100	R
61° 17°	20	St 709	1400 ± 90	I
59° 18°		St 1255	1380 ± 70	I
53° 10°		Hv 558	1310 ± 60	M

It is interesting that deposits of tangue at four separate locations in the north of France all date from this 400 year period between the transgressions of N and O. Tangue is a loose sediment consisting principally of silty sand, with clay and abundant remains of marine shells (40 to 48% by weight). It is characteristically overlain by sand dunes or forms outcrops on the strand. Labeyrie suggests that Gif 387 at least may be eolian, but Giresse and others (Radiocarbon XI ii, p.328) suggest that it is a direct marine deposit. The way the dates coincide with the end of the evidence of transgression (Gif 391) and the N/O regression (Gif 387, 389, 388) would seem to suggest that the deposit may well be related to a reduction in marine influence, whichever explanation is accepted.

48° 1°W	199	Gif 391	1680 ± 120
48° 1°W		Gif 387	1470 ± 120
48° 1°W		Gif 389	1430 ± 120
48° 1°W		Gif 388	1250 ± 120

(x) Phases O and P to the present

According to the Difference Curve, Phase O begins about 1400 B.P., reaches its maximum about 1150, and ends around 850 when it is succeeded by Phase P. This in turn reaches its maximum about 450 B.P. and ends about 350 B.P. The curve then rises towards the present day.

Less than 30 radiocarbon dates fall after 1400 B.P. Some 14 of these fall in the period designated Phase O, 9 in P and 6 between then and the present day. Phase O is represented by only three multi-date sites (sites 20, 112, 141) and P by only two (66 and 81). None of these suggest more than one episode of transgression in the Phases concerned.

The shortage of radiocarbon dates in this period, compared with the majority of the Holocene, limits the value of any conclusions reached on that basis. The fact that important perturbations of the radiocarbon input curve are known to occur within the last thousand years (viz. Chapter 5) further exacerbates matters. However, by this time contemporary or near contemporary written accounts of changes in marine influence on the western seaboard of Europe are becoming abundant. In general, then, for the majority of this period historical accounts of coastal change would appear to offer greater potential for investigation than radiocarbon determinations. This is however beyond the scope of the present study. As noted in (ii) above, the range of Scottish archaeological material of interest here does not extend into the present millennium, and this period will accordingly not be discussed in detail.

Dates relevant to the regression between Phases N and O have already been listed in (ix) above. It will be recalled that of these, St 709, St 1255 and Hv 558 fell after 1400 but before 1300 B.P.

The dates which appear to define the onset of the transgression of Phase O most precisely are:

54° 12°		Bln 214	1265 ± 100	M
54° 8°	115	K 797	1180 ± 100	T
54° 9°	112	Hv 231	1090 ± 90	T

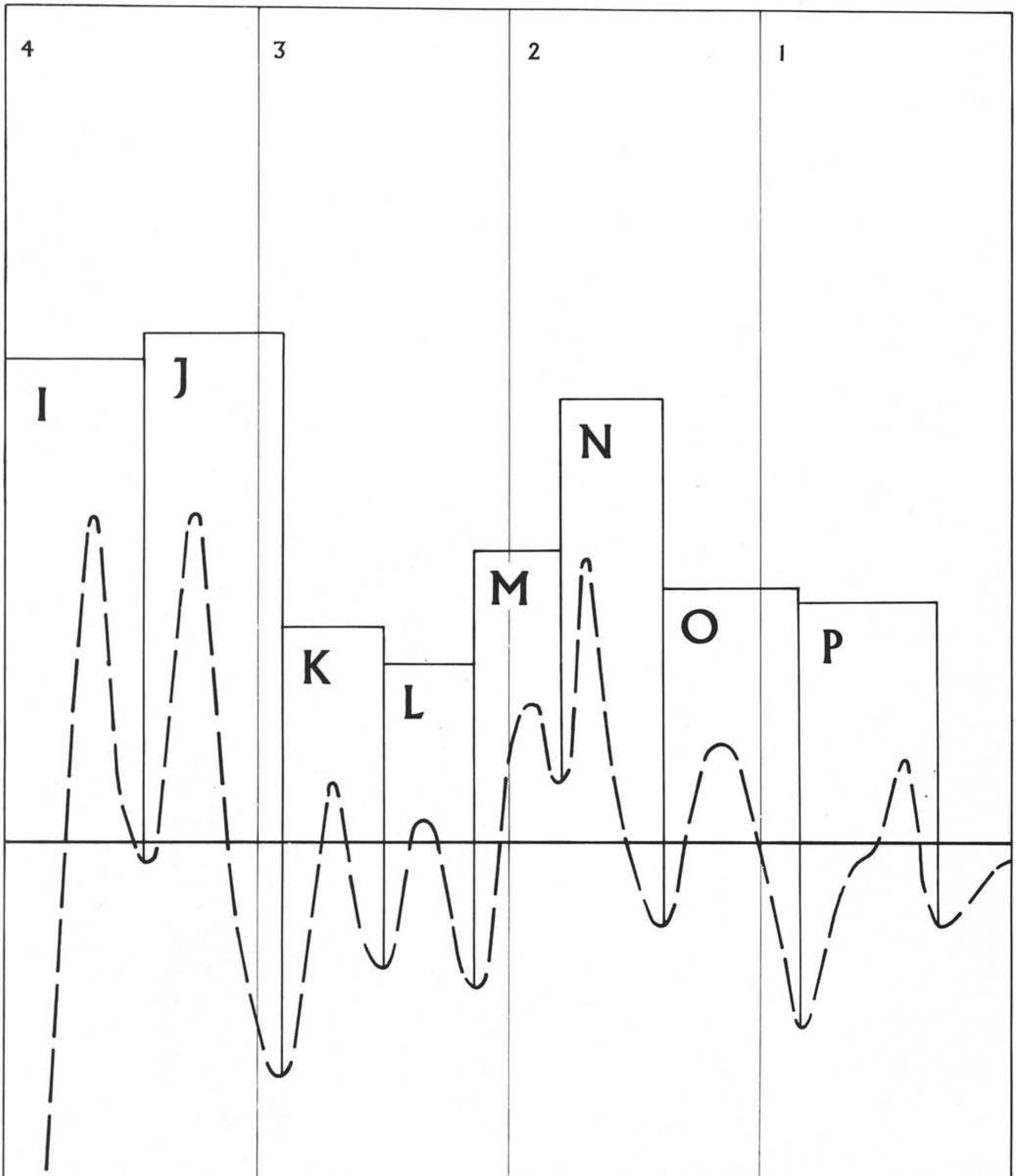
In the case of site 115, other dating evidence suggests that transgression went on until 950 B.P. This appears to conform well with the radiocarbon dates that appear to define the end of Phase O most closely:

61° 17°	20	St 776	945 ± 130	I
70° 30°		T 245	850 ± 80	R

The accumulation of general Class S evidence apparently corresponding to the regression after Phase Q has already been noted in (ii) above.

As previously stated, the conclusions from the present chapter will be summarised along with those from Chapters 8 and 9, in the chapter which follows.

Figure 10.1 Identification of Phases between 4000 and 0 B.P. in terms of the Difference Curve.



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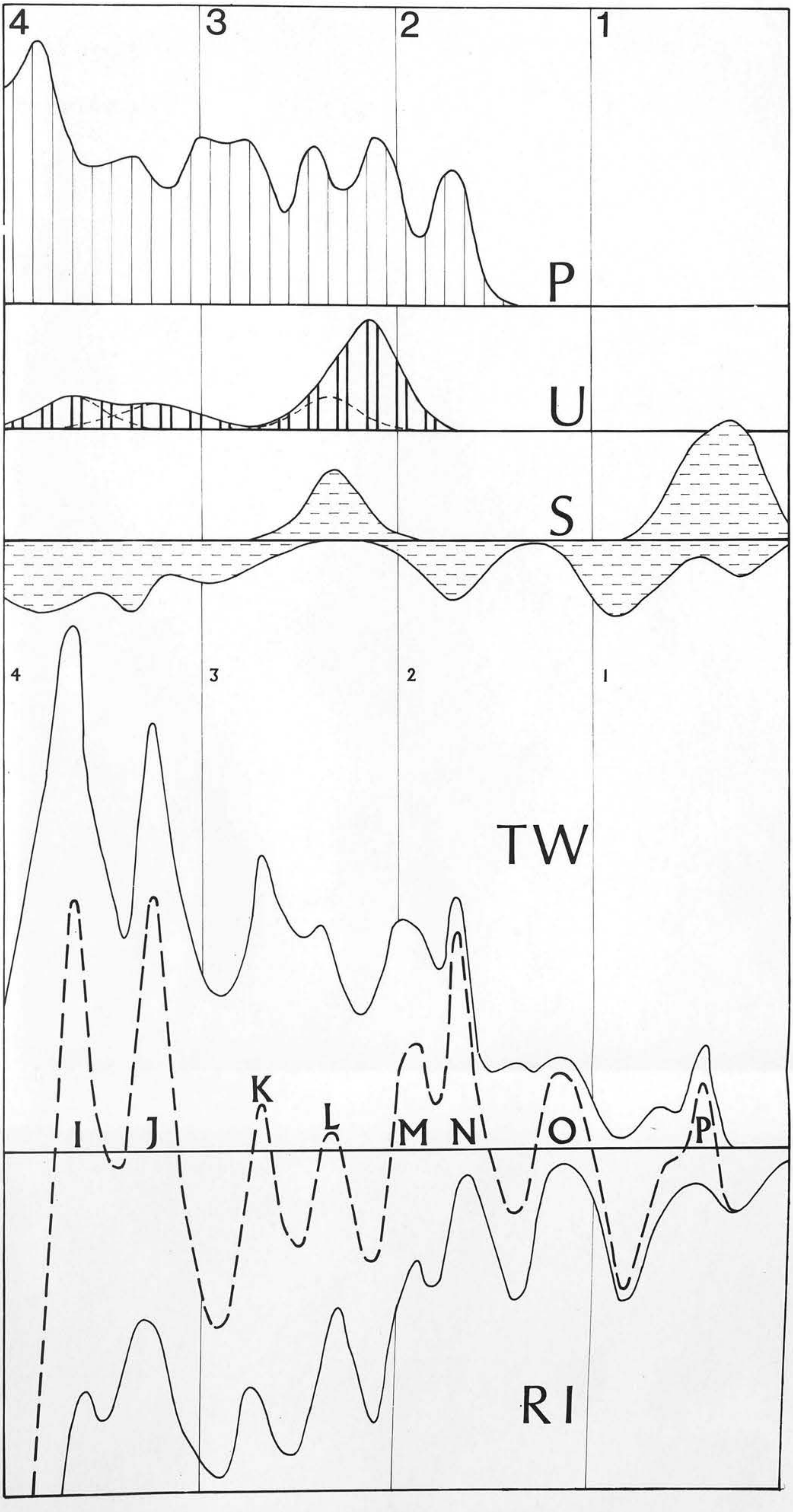
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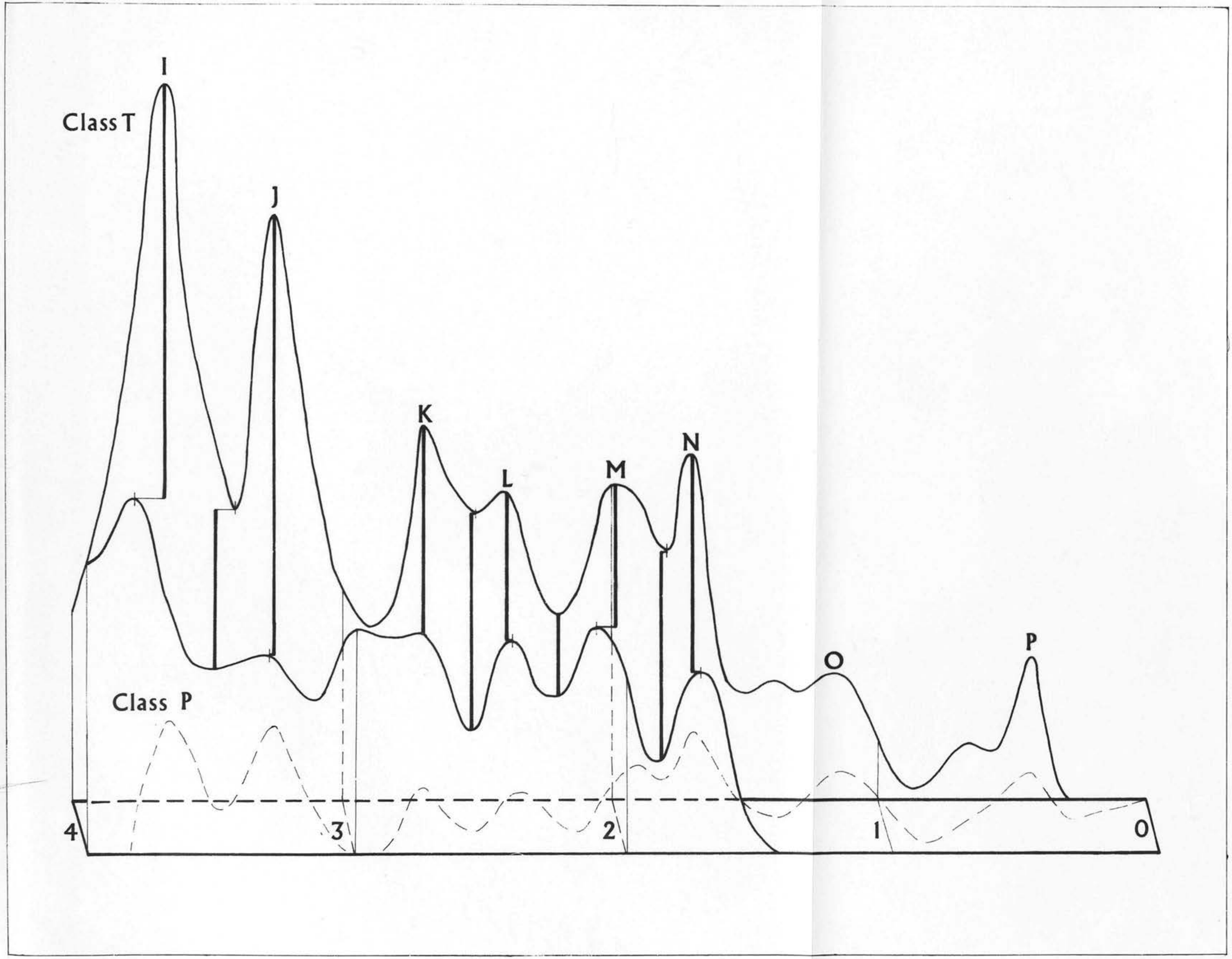
Figure 10.3 Class P curve displaced 50 years relative to both Class T and the Difference Curve (with the latter shown at $\frac{1}{4}$ vertical scale, as a dashed line).



Class T
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Class P
4
3
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1
0

Chapter 11

The distribution of the evidence, through time and in terms of geography

Introduction

In Chapters 8, 9 and 10 the radiocarbon determinations from beyond Scotland were considered as a whole and the validity of each phase in the hypothetical sequence that had been derived in Chapter 6 was examined. In the opening part of the present chapter the results of that examination of the time pattern of the dates will be summarised (Section I).

Then in the main part of the chapter (Section II), the extent of the geographical distribution over which each episode is represented will be discussed. The range of altitudes at which the evidence now lies and the variety of different types of ancient coastal environment registering the changes will be noted and conclusions summarised in Section III.

The aims in this are twofold. The first is to provide a specific but reasonably concise basis for the discussion of the available Scottish evidence that follows in Chapters 13 and 14. The second is to provide a starting point for the construction of a model of Holocene eustatic change. This model will be proposed and discussed in Chapter 12, and then applied experimentally to the Scottish data in Chapters 13 and 14.

I. The time-pattern of the radiocarbon determinations

When the initial hypothesis proposed in Chapter 6 is viewed in terms of the results of Chapters 8, 9 and 10, it would seem that there is clear evidence of at least three major and distinct periods of increasing marine influence in the Holocene prior to 6000 B.P. (B, C/D and E) and some suggestion that as many as six episodes of transgression may in fact be represented in this period.

Between 6000 and 4000 B.P. as many as four phases of transgression are possible, although, if this is so, two of these were certainly minor events.

In the most recent four millennia, there may have been as many as nine phases (if one is considered to be in progress at the present day). The apparent rapidity of the oscillations of sea level in this period, together with the decrease in the number of radiocarbon dates available, paradoxically renders conclusions on this basis less secure as the historical period is approached. As noted in Chapter 10, it is unnecessary to consider the most recent millennium in the present study, and in the preceding three millennia it seems preferably to distinguish only five separate phases, although some of these exhibit subdivisions.

The time-pattern of the changes will now be summarised, phase by phase.

Although evidence for Phase A is not plentiful, it appears to be consistent with a single phase of transgression reaching its maximum about 10000 B.P. with evidence of regression then building up rapidly just prior to 9750 B.P. There is then little evidence of either major increases or decreases of marine influence for almost four centuries. The evidence of transgression in Phase B is thus separated from the previous grouping by a clear half millennium.

In Phase B, the period from 9400 to 8750 B.P. is dominated by evidence of increasing marine influence. Indeed no radiocarbon date referring to a decrease in marine influence falls within this period. From 9100 onwards, however, the evidence of transgression is markedly less plentiful, and from about 8700 to almost 8300 B.P. the trend is completely reversed, with all positive evidence outside the Baltic referring to regression. According to the evidence cited in Chapter 7, this was the period when the Baltic became isolated from the ocean and was converted into the "Ancylus Lake". Phase B appears to represent a single episode of transgression, and the 400 year length of the succeeding regression separates it effectively from the next period of increasing marine influence.

Because of the unusually high incidence of inversions of date order relative to stratigraphic order between 7750 and 7500 B.P., no conclusion can be reached for the moment as to whether Phases C and D represent distinct episodes of transgression. However it seems clear that neither of these proposed Phases offers grounds for any further subdivision. From about 8300 to 7750, there is heavy dominance by transgression evidence (C). This is renewed from 7500 to 7000 B.P. (D), when it is replaced by evidence of regression which prevails in turn until just before 6600 B.P. The transgression of Phase E is thus separated from the earlier transgressions by about four centuries of decreasing marine influence.

Phase E begins with a period in which transgression evidence is completely dominant. This lasts from about 6650 to 6400 B.P. There is then some suggestion of a minor regression, terminated by a slight transgression about 6250 B.P. Regression then again sets in, and prevails until 6050, when the marked increase of marine influence distinguished as Phase F begins. This lasts until 5930 B.P., when it is in turn replaced, until 5300 B.P., by regression.

This 600 year period of decreasing marine influence effectively separates the transgressions of Phases F and G. The latter started just after 5300 B.P. and prevailed until between 5000 and 4900 B.P.

There was then apparently a brief alternation of regression and transgression. The regression lasted only to 4800, and the transgression (Phase H) until just after 4600 B.P. Although the brevity of this episode casts doubt on the validity of distinguishing between G and H, it will be recalled that the stratigraphy at widespread sites in fact appears to confirm that Phase H indeed represents a separate event of sea level change.

After Phase H, evidence of regression builds up markedly until it reaches a peak at 4300 B.P. Although evidence of reduced marine influence remains heavily dominant until the onset of the transgression of Phase I about 3850 B.P. (see Chapter 10), there would seem to be some reliable evidence within this period for a minor episode of transgression centred between 4300 and 4200 B.P.

The transgression of Phase I is well defined. In marked contrast to the heavy preponderance of evidence of regression prior to 3850 B.P., during the next 250 years evidence of transgression is definitely dominant.

Then after a brief regression, the transgression of Phase J starts at 3500 and prevails until 3150 B.P., when regression evidence again becomes dominant. This condition persists until about 2900 B.P. when the transgression of Phase K starts, to last for some 200 years.

From 2700 to 2450 B.P. there is an overall preponderance of evidence of regression, but some indications of transgression do occur. There is however no clear proof of an additional stratigraphically distinct episode at this period.

From 2450 to 1650 B.P. evidence of transgression builds up, with minor intermissions centred about 2200 - 2100 and 1900 - 1800 B.P. The substages of this transgression show peaks at about 2350 (L), 2000 (M) and 1700 (N).

After 1650 B.P., there is no major evidence of transgression for about 400 years. Evidence of regression dominates this period. Then the transgression of Phase O appears to reach its peak about 1150 B.P., before the onset of regression at about 1000 B.P. As noted earlier in the chapter, changes in the present millennium will not be discussed in detail.

Before proceeding further, one general point regarding the timing of the transgressions and regressions should be considered. It may have been noted from the graphs and descriptions given in Chapters 6 to 10, and also the present summary, that there would appear to be some indications of periodicity in the timing of these events.

That such a tendency may exist is not a new idea. Several workers have considered it in general terms, but the most specific examination of the concept would appear to have been made by Bennema in the Netherlands. His conclusions are stated most concisely in his paper of 1954.

In terms of the data included in the present survey, the tendency would appear to be most evident in the period between Phase I and the present day (e.g see Fig. 11.20 at the end of this chapter).

Such appearances of periodicity can however be misleading. It was accordingly decided to attempt to evaluate this objectively. Craddock (1968) has recently emphasised the value of Sherman's statistic "Omega" for helping to judge whether dates on which an event occurred in the past are distributed at random.

In calculating this, it is assumed that each event must happen on a particular date, and the interval between one possible date and the next is taken as the time unit. Then let $d_1, d_2, d_3 \ldots d_n$ be the dates on which the events actually occurred, given in chronological order, and d_0 is the earliest possible date and d_{n+1} the latest possible date. $D = d_{n+1} - d_0 + 1$ is the total time covered by the possible dates and the occurrences divide this into $n + 1$ intervals. Sherman's statistic is then defined by the equation

$$\text{Omega} = \frac{1}{2D} \sum_{j=1}^{n} \left| d_j - d_{j-1} - \frac{D}{n+1} \right|$$

The distribution of this statistic has been investigated by several workers, and Craddock (op.cit. p.171) provides a chart showing its percentiles for values of n from 2 to 50. The value of Omega is always between 0 and 1, and an improbably low Omega for the value of n involved shows that the dates are too regularly spaced for them to be due to chance.

It will be recalled from Chapter 5 that because the calibration of the radiocarbon timescale in true calendar years is as yet incomplete, to conform to current international practice all dates in the present study have been given as unmodified radiocarbon "years" on the Libby half-life.

The tendency to periodicity was first observed in the graphs drawn to that timescale, and the writer considered that the distortions of the radiocarbon scale relative to sidereal time (i.e. true "earth years") might be giving an illusory impression of regularity, particularly since the discrepancies between the two scales become fairly substantial in the earlier part of the period of interest (e.g. the peak of the Phase I transgression is dated 3640 B.P. in "radiocarbon years", but 4350 B.P. in "earth years", according to the latest available calibration figures: see below).

In contrast to the earlier part of the Holocene, calibration data are already substantially complete for the more recent millennia. It was accordingly decided that it would be feasible to carry out the assessment of periodicity in terms of a fair approximation to real time, rather than the distorted radiocarbon scale.

The calibration of the dates was effected by using the sidereal conversion equation first published by Stuiver and Suess in 1966, but incorporating the modification suggested by Stuiver in 1969 (in Scholl, Craighead and Stuiver, 1969). It was further refined in terms of his graphs (op.cit.) to make allowance for the non-linearity of the correction. Stuiver recommends that allowance should be made for the ratio of the Libby and "best" half-lives (5568:5730) before embarking on the remainder of the conversion procedure, and this was done.

The "Omega" values were then calculated for the dates in earth years of the maxima of transgression and regression evidence, as defined by the Difference Curve for Phases I, J, K, L, M, N, O, P to the present day. Three calculations were made. The first was for the dates of transgressions, the second for the dates of regressions, and the third for the combination of the transgression and regression dates. The respective values of Omega were 0.126, 0.124, and 0.123. When these values were each compared with the distribution of the statistic in terms of the appropriate value of n, it was found that in all cases the likelihood of a periodicity as regular as that observed arising in the dates by chance was substantially less than one per cent.

It would thus seem that the impression of periodicity in the evidence has in fact an objective basis.

It is interesting that, during the time between Phase I and the present day, the average period between the peaks of transgression evidence is circa 545 years. Earlier in the Holocene, this is approximately the period between the peaks of E and F, between H and the succeeding unnamed minor transgression, and between that transgression and I, while the F to G period is near to twice this average length.

The close agreement with Bennema is also notable. He too concluded that the period separating transgressions was between 550 and 500 years in the latter half of the Holocene. His data were drawn essentially from the Netherlands. The fact that on the evidence of the present survey this tendency would appear to be confirmed across the whole spectrum of the western European seaboard would seem to narrow the range of hypotheses that might account for the periodicity. A general consideration of these possibilities lies beyond the scope of this thesis, but aspects of the matter particularly relevant to the present investigation will be considered further at a later stage.

II The geographical range over which the transgressions and regressions are represented

The maps used in this chapter all refer to units of time defined in the detailed examination of the evidence in Chapters 8, 9 and 10, and summarised in Section I of the present chapter. Only periods with a clear tendency towards domination by either transgression or regression evidence are mapped (for example, the periods between 9750 and 9400, and 7750 and 7500, when the tendency was unclear, have been omitted.)

On the maps, for ease of reference, the distribution has been shown in modules one degree of latitude by one degree of longitude, since it is unnecessary to show the precise position of individual sites in order to judge whether a distribution is widespread enough to imply eustatic control. Where possible, both a period of transgression and the succeeding period of regression are shown with different symbols on the same map (cases where this is not so are distinguished in the text.)

Within each one degree module, the symbol used for the occurrence of evidence of transgression is a triangle, with the hypotenuse rising from left to right. Regression is signified by a triangle with the hypotenuse instead falling from left to right. These symbols are superimposed when evidence of both types occurs in the same module. (Since this means that both a transgression and its succeeding regression period are represented there, the superimposition thus signifies confirmation not conflict.) Where the evidence is clear, the triangles are filled in with solid black. Open triangles show that the evidence is less definite.

The basis for this classification of the evidence is given in detail in Chapters 8, 9 and 10 where (as noted in the Introduction to Chapter 8) the coordinates of the individual sites plotted on the maps of the present chapter are given, Phase by Phase, as the dates characteristic of each alternation of transgression and regression were listed.

The Distributions

The geographical distribution of the sites that figure in Phase A is shown in Fig. 11.1. It will be noticed that almost all the evidence from this phase comes from Scandinavia.

This northerly emphasis on the distribution of available evidence is characteristic of the earlier parts of the period under study. To the south, outside the areas of substantial glacio-isostatic uplift, the early Holocene shorelines are now deeply submerged well beyond the present coast, and are thus relatively inaccessible. With Scotland and, in this period, the Baltic out of consideration (because of possible overlap with the "Baltic Ice Lake") a distribution restricted to Norway and the Kattegat must thus be expected in terms of the physical availability of samples for dating.

The particular concentration on the Kattegat reflects recent interest in that area on the part of the Swedish Geological Survey, together with a general contrast in the relative availability of radiocarbon dating facilities in Sweden (cf. Stockholm/Uppsala and Trondheim date lists in the Appendix). The value of this distribution map in geological terms is thus limited.

The fact, however, that all the sites are located in areas influenced by glacio-isostasy raises the possibility that the time pattern represented by the dates of Phase A might reflect the timing of land uplift, rather than eustatic control. The Sogne Fjord and Oslo Fjord sites are certainly involved in local glacier variations. On the other hand, it might be argued that the close agreement of the dates of maximum transgression and regression at sites over a geographical range from Denmark to North Norway, with conditions varying from the shelter of the Kattegat to the exposed Arctic coast, suggests as widespread a control as eustatic sea level. The possibility also exists that both factors are involved, synchronised by a climatically controlled variation in glacier volume. The writer considers that insufficient information is available within the bounds of the present survey for a satisfactory decision to be reached.

It may be noted at this stage that as many Scandinavian workers have pointed out (for instance, in the literature cited in Chapter 7), in much of the central and north Baltic, land uplift was so rapid throughout the greater part of the Holocene that there tended to be a continuous overall fall in relative sea level there. As the maps that follow show, radiocarbon dates referring to transgression contacts are accordingly rare in this area. On the other hand, as will have been noted from Chapters 5 to 10, Class I (Isolation) dates are correspondingly plentiful there and, as has been shown, in general these conform well to the timing of other indications of reductions in marine influence elsewhere.

The geographical distribution of the sites that figure in Phase B in general reflects the same limitations as that of Phase A, and for the same reasons. Two extensions to the distribution are however apparent (Fig. 11.2).

Firstly, although the sea was still well beyond the Dutch coast at this time, a watertable rise that Jelgersma (1961) considers may reflect the earliest influence of the rising sea there occurred at Uitgeest ($52^{\circ}4^{\circ}$) at a site now buried well below present sea level. (The implications of the altitudes of the sites are assessed in Chapter 12.)

Secondly, although the "Ancylus Lake" stage renders inadmissible the Baltic evidence that coincides with the regression that terminated Phase B on the seaboard outside, the preceding transgression itself is admissible (Chapter 7) and this is registered widely in the south Baltic ($54^{\circ}10^{\circ}$; $55^{\circ}12^{\circ}$;

$55^{\circ}15^{\circ}$; $56^{\circ}15^{\circ}$.)

Despite these extensions, the limitations on the available evidence still make it difficult to reach any firm conclusion from the map distribution alone as to whether the sites involved in this Phase reflect eustatic control. However, some four times as many radiocarbon dates are available for this period, as compared with Phase A, and the internal consistency of the pattern of these ca. 90 dates certainly carries some suggestion that this may be so. Furthermore, as noted above, according to the conclusions reached in Chapter 7, this transgression was apparent in the Baltic as a marine phase ("Yoldia Sea"), while the succeeding regression saw the Baltic's isolation as the "Ancylus Lake". This suggests that both the transgression and regression of Phase B were probably of more than local importance, although land movement clearly had an important role in the changes.

In the case of the transgression of the period designated Phase C, little room for doubt remains (Fig. 11.3). The flooding of the North Sea basin had by then proceeded sufficiently to be detectable within the limits of the present coast of the Low Countries, albeit under a heavy overlay of later deposits. Evidence also occurs above present sea level in the isostatically uplifted area of Northern Ireland, while for much of this period the Baltic formed a direct extension of the ocean's surface. Evidence of this phase is thus available from the southern North Sea (and perhaps the English Channel) in the south to at least west Norway (indeed apparently the Arctic) in the north, and from Ireland in the west to the Gulf of Finland in the east. Phase C would thus appear to suggest strong eustatic influence.

The problem of the distinction between C and D has already been discussed. Because this cannot readily be resolved, the evidence of C and D is is shown separately in Fig. 11.3 and 11.4, respectively, and then combined in Fig. 11.5. It will be seen that whether or not D is regarded as representing a separate event, the sites that fall within its scope are certainly also sufficiently widespread to suggest that that period of transgression as well as the period of regression that followed also reflect strong eustatic influence.

It will be recalled that examination of the sequence at individual sites (Chapter 8) suggested that it appeared advisable to subdivide Phase E. The main Phase E transgression (6650 - 6400) and the immediately succeeding regression are shown on Fig. 11.6, while the minor transgression that followed at 6250 B.P., together with the ensuing regression, are shown on Fig. 11.7. It will be seen that the main episode is represented from the English Channel to Arctic Norway, and from Ireland to Finland. It would thus appear to be eustatic.

The minor transgression is dated directly at only four sites, so it is certainly possible that the conjunction of these dates is a coincidence, and that all these sites represent merely anomalous local conditions.

As Fig. 11.7 shows, the build-up of regression evidence that succeeded the apparent minor transgressions occurred over a wide front, from Wales to Finland and north Norway. It thus certainly seems to reflect eustatic control. However, if the concept of a minor eustatic transgression at circa 6250 is considered invalid, this distribution of regression evidence can equally well be regarded as the continuation of the regression terminating the main episode E, and it can not thus be regarded as proof of the existence of a second eustatic transgression in this period.

On the other hand, it will be recalled that Morner (1969) was sufficiently impressed by the evidence in the Kattegat for a double transgression maximum (his PTM3A and 3B), within the present writer's Phase E, for him to include this on his eustatic curve. The distribution of the evidence : southwest England ($51^{\circ}3^{\circ}W$), Kattegat ($56^{\circ}12^{\circ}$), north Norway ($68^{\circ}17^{\circ}$) : is at any rate not inconsistent with eustatic control, if his viewpoint is accepted.

No firmer conclusion on the latter part of Phase E seems advisable at this stage, but when Phase F is considered (Fig. 11.8) the evidence once more appears to be clearly in favour of eustatic control, in that both the transgression and the ensuing period of regression is again well represented, across the whole range of the seaboard from the Irish Sea as far as the east Baltic and Arctic Norway. The same conclusion seems justified for Phase G (Fig.11.9), represented from Ushant to North Cape in the Arctic, by way of the North Sea and Baltic.

In the case of Phase H, however, it will be recalled that Class T and W evidence of transgression was limited in amount. However, it was shown in Chapter 9 that the stratigraphy of at least 16 widely dissipated sites showed specifically that H in fact represented a separate event of transgression from G. In Fig. 11.10, the occurrence of Class T and W evidence is shown by a solid triangle in the usual way, but the additional evidence of these other sites is shown by open triangles. It will be seen that on this basis, the evidence extends from the English, Dutch and German North Sea coasts to central Sweden and south Finland. It would thus seem that although the transgression period H was certainly a minor one, it quite probably reflected eustatic control.

The succeeding regression was widespread. The part of it between the end of H at 4600 and the peak of the RI curve at 4300 is shown on Fig. 11.10. It is represented in both the west and east of England, in the German Bight, Baltic and in Arctic Norway as well as Holland. Thus although the size of this peak on the RI graph is somewhat inflated, as was noted in Chapter 5, by the interest of Dutch workers in the date when the main Upper Peat buried in their

Tidal Flat deposits started forming, there is no doubt that it represents more than a local event, and the range of the distribution strongly suggests that it was an eustatic regression.

It has been suggested above that, despite the overall prevalence of evidence of regression between 4600 and 3850 B.P., there appears to be some reliable evidence suggesting a brief transgression centred between 4300 and 4200 B.P. The distribution of this evidence is shown in Figure 11.11. The most diagnostic evidence of transgression and regression is shown in the usual way, with solid triangles. In addition in this case two extra symbols are used. The one degree modules containing a cross indicate the occurrence of sites with stratigraphy strongly suggesting a brief additional transgression in this period, while those that are outlined but not infilled contain other sites giving general although not definitive support.

It will be seen that even if the latter two categories are disregarded, the distribution extends from Biscay, through the English Fenland and the eastern North Sea coast to the Kattegat and Arctic Norway. In this case however the small number of radiocarbon dates and sites involved, compared with the great majority of the Phases, make it necessary to question any attribution to eustatic change particularly closely. The need for a particularly full discussion is emphasised by certain characteristics of the distribution.

It will be noticed for instance that the Arctic and Biscay dates are for regressions, and that the positive transgression contact dates are concentrated round the North Sea (viz. Chapter 9). This raises the possibility that the episode might reflect not eustatic change but some effect characteristic of the North Sea environments of deposition.

Compaction is one possibility. The depth of Holocene sediments at several of the sites involved is such that local transgressions due to settling of the subjacent peats and clays is conceivable (e.g. 138 $53^{\circ}7^{\circ}$; (Q474) $52^{\circ}0^{\circ}$; 119 $54^{\circ}8^{\circ}$). It is however highly unlikely at others (e.g. 140 $53^{\circ}7^{\circ}$; 174 $51^{\circ}4^{\circ}$) where the material dated rested directly on Quaternary material that Jelgersma (1961 and 1966) and others accept as highly resistant to compaction.

Tectonic subsidence of the North Sea geosyncline might be invoked, but this is not consistent with the Kattegat evidence, since isostatic uplift was certainly in progress there at that time, according to Morner (1969) and others. The near tideless nature of the Kattegat, then, as now, would also seem to prevent North Sea storm surges from serving as a general explanation.

It will be recalled from Chapter 9 that Marthinuusen was able to distinguish the Artic episode at this period as a separate minor shoreline (his N4 line), while the stratigraphy at Site 202 in Biscay also appeared to give a clear indication of a distinct brief transgression at this time.

On balance, then, it would seem not unlikely that the widespread distribution of the evidence does indeed reflect a slight eustatic fluctuation.

Nevertheless, although the Class R and I evidence decreased during the period of this possible transgression, as pointed out above, evidence of regression in fact dominated the whole period between 4600 and 3850 B.P. The distribution of this evidence between 4600 and 4300 has already been shown (Fig.11.10). For completeness, and in case the above conclusion regarding a proposed brief eustatic transgression is in error, the distribution of all evidence of regression between 4300 and 3850 B.P. (including that attributed to the brief transgression on stratigraphic and morphological grounds, and thus shown in Fig. 11.11) is shown in full in Fig. 11.12.

The succeeding transgression/regression cycle, Phase I, is shown in Fig. 11.13. It will be seen that despite the common lack of transgression contact sites from Scandinavia, evidence from west Britain, including Ireland, would appear to rule out any purely regional North Sea effects as the cause of the transgression between 3850 and 3600. The representation of the ensuing regression period in widely contrasting environments from the English Channel by way of the North Sea coast to the Baltic and north west Norway suggests that this was also most probably eustatic, despite its brevity (less than 250 years) and the relatively small number of radiocarbon dates involved.

The same remarks hold good of the transgression of Phase J, which lasted until ca. 3150, together with ensuing regression (3150 - 2900 B.P.). These are shown in Fig. 11.14, and again it seems reasonable to accept them both as eustatic events. Indeed, in this case, the regression is notably well represented across the seaboard.

This is not however so of the transgression of Phase K (2950 - 2700), shown in Fig. 11.15. The distribution of transgression evidence is restricted to the southern North Sea, and as indicated above, it might thus be explained in terms such as compaction or storm surge. This and similar cases which follow will be discussed further, as a group, at a later stage in this section.

Little doubt however surrounds the likelihood of eustatic control in the regression that took place between 2700 and 2450 B.P. (also shown on Fig. 11.15). It will be seen that this is apparent from Biscay to the Arctic, and from Ireland to the Baltic, as well as on both sides of the North Sea and in the Channel.

Fig. 11.16 shows the transgression of Phase L (2450 - 2200) and the ensuing regression. In this case it will be seen that with the exception of the Biscay evidence the distribution of the transgression evidence is again essentially a North Sea one, although slightly more dispersed, while in complete contrast the evidence of regression from the period between L and M is restricted to three one degree modules, all in Fennoscandia. Both these characteristics will be investigated further at a later stage.

The transgression of Phase M is more widely evident. This is shown along with the succeeding very brief regression in Fig. 11.17. It will be seen that the distribution of transgression evidence extends from the North Sea area to both sides of the Channel, and also to south Baltic Sweden. The succeeding regression is again based on minimal evidence (only three radiocarbon dates), and the transgression of Phase N (1800 - 1650) (Fig. 11.18) is also restricted to the North Sea and French Channel coast. The regression that follows (1500 - 1300) is however evident on both sides of the North Sea as well as in the Baltic.

The number of sites with radiocarbon dates available in the period designated Phase O is too small for useful conclusions to be drawn from the distribution map (Fig. 11.19), and the investigation is accordingly stopped there.

General Discussion

As indicated in Chapters 5 and 6, by showing a strict alternation of periods in which evidence of increase and then decrease in marine influence is successively dominant, the TWRI graphs appear to indicate that a strong measure of eustatic control is present, tending to synchronise the transgressions and regressions at the individual sites included in the survey.

It will be recalled that Classes W and I were drawn from relatively restricted geographical ranges (the eastern North Sea coast and Fennoscandia respectively). These areas, however, between them embrace the extremes of the spectrum of Holocene land movement believed to exist on the western seaboard of Europe (viz. Chapter 4). Furthermore, the majority of the dates involved in those graphs (Classes T and R, 68% of total) were drawn from the whole geographical range of the seaboard. They thus come from such a wide range of geophysical conditions as to make it distinctly unlikely the time-pattern evident in the changes could be imposed by systematic variations in land movements. Although various workers in the Low Countries (quoted by Jelgersma, 1961) have suggested that rates of compaction and subsidence probably varied from time to time there, unless hydroisostasy tied to eustatic change was the mechanism, it is difficult to see how any short term changes there could be related in detail to variations in land uplift in Scandinavia. In any case, as already noted, Lundqvist (1965) has pointed out that majority opinion in Scandinavia favours smooth land movement there in the Holocene.

It thus seems improbable that the rapid and widespread succession of synchronous changes observed in western Europe can be explained in terms of fluctuations in land movement.

It seems even less likely that the strict alternations of dominance apparent on the graphs represent merely fortuitous coincidences in the timing of local events. The high level of complexity at which the different classes of evidence match each other would appear to render this particularly improbable. In Chapters 8, 9 and 10 it was shown that the other categories of positive evidence (Classes P, U, S, and M) contained little to conflict with the overall pattern apparent from Classes T W R I, and much to support it. Thus, the sequence of alternations is supported by evidence drawn from the order of five hundred individual sites on the western seaboard of Europe. This would appear to minimise the possible role of chance in producing the overall pattern summarised in Section I of this chapter.

In general, the distribution maps would appear to support the view that the alternating periods of transgression and regression distinguished in

Chapters 6 to 10 do indeed reflect the synchronising influence of eustatic control. In most cases, the episodes are represented over a sufficiently widespread and varied range of the western European seaboard to make it difficult to find a convincing alternative explanation.

However, although it would seem reasonable to conclude that the overall space-time pattern of the evidence is primarily a reflexion of eustatic influence, this does not eliminate the possibility that certain episodes were of only local or regional importance. This must be examined with particular reference to the latter part of the Holocene.

It would seem that, from the end of the regression following Phase J, the evidence shows two apparently divergent characteristics with respect to the possibility that the changes reflect eustatic control. The number of dates representing some of the transgressions and regressions in this period is small, but while this emphasises the need for caution in reaching conclusions, as will be shown below it does not of itself appear to account for the pattern apparent in the evidence. This appears too systematic to be explained simply in terms of the chance incidence of a limited amount of data.

The two apparently divergent characteristics of the evidence are brought out most clearly by the graphs and maps respectively. The graphs appear to support a measure of eustatic control, while as noted above several of the map distributions from Phase K onwards suggest alternative explanations for the evidence.

The relevant graph information is summarised in Fig. 11.20. For compactness the ordinate scale has been reduced to a proportion of one quarter of that customarily used relative to the abscissa scale. On this figure the top graph shows the TW and RI curves, together with the Difference Curve. The middle graph shows the extent of the amplitude of the oscillations of the Difference Curve, relative to the total amount of Class T, W, R and I evidence. The lowest graph re-expresses this statement of the "signal-to-noise" ratio of the evidence (cf. Chapters 5 and 6) by showing the effect of subtracting the area enclosed between the Difference Curve and the abscissa from the T W R I total.

It will be seen that although for much of the period of interest the "signal to noise ratio" is less favourable than in the majority of the earlier Holocene, nevertheless the TW and RI curves continue to show closely corresponding fluctuations. Maxima on the TW curve are still reflected in minima on the RI curve, and vice versa.

Yet although as these graphs indicate, and Chapter 10 established in detail, the timing of transgressions and regressions after 4000 B.P. remains intimately interlocked in a sequence of alternations, the maps show that at least in some Phases the transgression and regression evidence involved comes from contrasting geographical distributions. The periods in which this is clearest are the transgressions of Phases K, L and N, and the regressions after L and M (but not K and N). In the case of Phase L (Fig. 11.16) the contrast is so marked that the distributions of transgression and regression evidence are mutually exclusive.

In all these cases, the tendency when only a limited amount of transgression evidence is represented is for this to come from the southern shores of the North Sea, where the sequence was dominantly transgressive during the entire Holocene. Correspondingly, when only a limited amount of regression evidence occurs, this tends to come from Fennoscandia and in particular from the central Swedish area of high isostatic uplift, where a dominantly regressive sequence is apparent throughout the Holocene. A comparison of Figs. 11.15 to 11.18 with the map of apparent present-day European land/sea movement (derived from Tidegauge records and given in **Dury 1966**) will show that the current zero line effectively separates the core areas of the two geographical tendencies in the distribution pattern.

As has been shown earlier, (for instance in Chapters 5 and 6, and in discussing Phase B in the present Section), this imbalance in the distribution of the evidence is not restricted to the part of the Holocene at present under consideration. It is for instance present throughout in the Class W and I evidence, respectively "North Sea / transgressive" and "Fennoscandia/regressive". Yet it has been shown that the fluctuations of the graphs of these classes relate to those of the more widely distributed Classes T, R, P, U, M. Thus, in the cases of Classes W and I, it seems clear that although the geographical distribution of the evidence reflects different conditions of depositional environment and land movement, the time distribution in general reflects the same synchronising influence as the rest of the Classes.

On this basis, the attenuated distributions shown in Figs. 11.15 to 11.18 might be attributed merely to the combination of this longstanding imbalance with the reduction in the number of dates representing the transgressions and regressions in question.

This would however appear to be an oversimplification. As the middle graph of Fig. 11.20 shows, what is involved is not an overall reduction in the number of dates available, but a series of inequalities in the representation of transgression and regression. It will be seen that in fact from 4000 B.P. until after 2000 B.P., the total number of Class T W R I dates remains at a level that compares quite favourably with the rest of the Holocene. The contrasting areas shown under the segments of the Difference Curve however reflect the inequalities in the evidence for the different transgressions and regressions. (These were set out in detail in the date lists of Chapter 10.)

The wholly North Sea distribution of evidence of transgression in

Phase K, and perhaps also the evidence of the transgressions in L and N (although this is somewhat more widely distributed and environmentally varied) might individually be explained in terms of non-eustatic factors. The same is true of the regressions registered between L and M and between M and N, because of the small numbers of dates involved and their representation only in Scandinavian areas of isostatic uplift.

However, although any of these cases might be accounted for <u>individually</u> in local or regional terms, the fact that their timing forms part of an interlocking sequence of alternating episodes of transgression and regression makes this difficult to accept. Indeed, the fact that events in geographically and geologically distinct part of the seaboard form integral parts of a sequence that has been shown (Section I) to exhibit a degree of regularity that has less than one prospect in one hundred of being due to chance, suggests strongly that these events too are synchronised by the same controlling influence as the other events in the same sequence. As has been suggested above, these other events certainly appear to reflect eustatic control.

It is therefore suggested that taking the stratigraphy, graphs and map distributions together, it seems not unlikely that after the widespread regression marking the end of Phase J, there was something approaching an eustatic stillstand. Transgressions were registered only in vulnerable sites in the southern North Sea area. Tectonic subsidence may have played a part there, but at at least two sites (<u>140</u>; <u>173</u>) compaction seems unlikely to have occurred, although it seems quite likely to have been an element at others. Most of the sites were also in locations vulnerable to storm surges. On the other hand, the shortage of regression evidence even in parts of Scandinavia undergoing uplift at this time suggests that the stillstand was not completely stable and a small overall rise may have taken place.

The periods between L and M, and between M and N, would similarly appear to have been near to stillstand, although in this case with a slight tendency towards an overall fall, so that not only did sites undergoing land-uplift rise clear of the sea but there was simultaneously a falling-off in evidence of transgression throughout the seaboard, including the areas of possible subsidence.

This concludes the main discussion of the geographical distribution of the data. The topic of time-transgressive shorelines remains. The discussion of this will however be reserved until Chapter 14.

III. Conclusions on the space-time distribution of the data

The coastal environments represented in Scotland in the Holocene range from the exposed western and northern coasts and the open North Sea shores, to sheltered sea lochs and protected muddy estuaries such as those of the Forth, Tay, Clyde and Solway. Despite the diversity of this range, it is outstripped by the variety of the environments represented at the sites elsewhere on the western seaboard of Europe that figure in the present survey. These range from exposed Arctic coasts on islands off Finnmark to coastal swamps in Biscay; from clear fjords to the muddy estuaries of Elbe, Rhine, and Maas; from the tideless Kattegat to the storm-surge tract of the southern North Sea; and include a considerable variety of Baltic conditions.

In the same way, the heights of the European sites cover a considerably greater range than is represented in the Scottish evidence. For instance, the highest Holocene shorelines of Scotland appear to reach only about 15 metres above Ordnance Datum, while the deepest relevant Scottish radiocarbon samples come from only about 9 metres below high tide level. The present-day altitude range of heighted European sites included in this survey is fully an order greater than this, in that the highest sites are at over 220 metres above present sea level, and the lowest is now submerged by 36 metres. The maximum distortion of a synchronous ancient waterplane indicated directly by the data shows differential land movement in excess of 250 metres. These are the extreme values, but a substantial proportion of the Fennoscandean sites lie well above 15 metres, and a large number of sites on the remainder of the seaboard lie lower than 9 metres.

Despite these variations in coastal environment and the differential land movements, as Chapters 6 to 11 show, the European data conforms to a time pattern that shows a high enough level of coherence to suggest strongly that eustatic influence has been sufficiently marked to synchronise local episodes of transgression and regression across this range of conditions.

Since both the variety of Scottish coastal types and the vertical amplitude characteristic of the evidence in Scotland lie well within the spectrum represented on the western seaboard of Europe, it would seem reasonable to suppose that Scotland too should tend to reflect the same events.

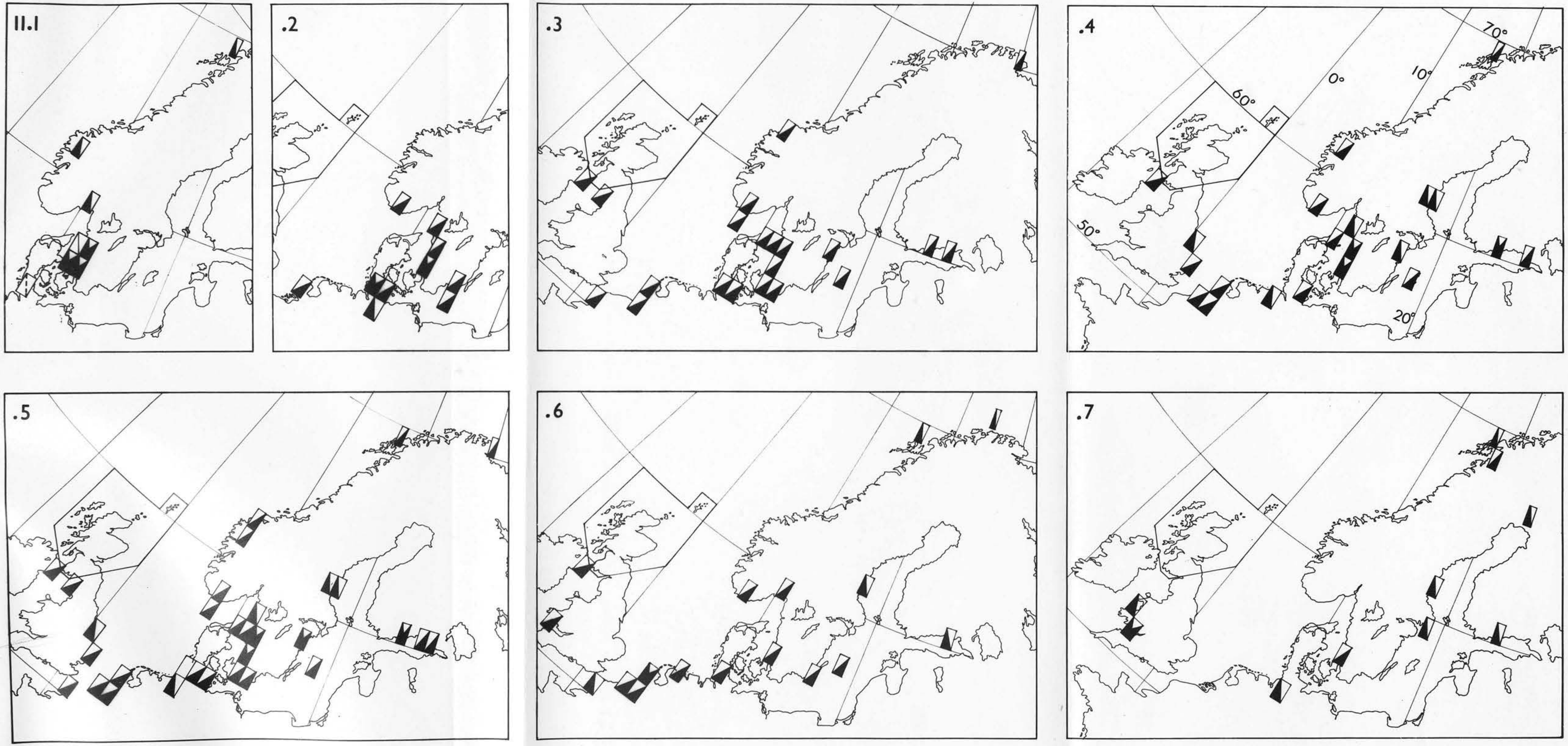
This will be investigated in Chapters 13 and 14, but first, as an aid to that discussion, in Chapter 12 an attempt will be made to construct a time/altitude model of eustatic change to supplement the essentially chronological model derived in Chapters 5 to 11.

The contents of this chronological model may be summarised as follows:-

10300 - 9750 B.P. :	Phase A: evidence indecisive - a possible eustatic transgression reaching maximum ca. 10000 B.P. and ended by a regression at 9750 B.P., but not proven.
9750 - 9400 B.P. :	Little evidence of either rise or fall of relative sea level.
9400 - 8750 B.P. :	Transgression of Phase B: a single episode, separate from those earlier and later. As with A, map distribution does not allow a firm decision as to whether or not eustatic, though internal consistency of ca. 90 dates and importance relative to Baltic suggest it may well have been. Rapid major transgression from 9400 - 9100, then reduces.
8700 - 8300 B.P. :	Regression : isolation of Baltic as Ancylus Lake. Again possibly but not definitely eustatic.
8300 - 7750 B.P. :	Transgression of Phase C: can not be further subdivided but because of confused evidence (inversions) between 7750 and 7500, not clear whether distinct from Phase D.
7500 - 7000 B.P. :	Transgression of Phase D: again can not be further subdivided. Although it is not clear whether C and D are distinct, C/D is separated clearly from earlier and later periods of transgression. Considered either individually or as a single unit, the C D period transgressions seem unambiguously eustatic.
7000 - 6650 B.P. :	Eustatic regression.
6650 - 6400 B.P. :	Phase E: major eustatic **transgression.**
6400 - 6050 B.P. :	Eustatic regression, perhaps broken at 6250 by a possible secondary Phase E transgression: latter according to Morner eustatic but not proven.
6050 - 5950 B.P. :	Phase F: a single, separate, eustatic transgression.

5950 - 5300 B.P. :	Eustatic regression.
5300 - 4950 B.P. :	Phase G: a single, separate eustatic transgression.
4950 - 4800 B.P. :	Brief but apparently eustatic regression.
4800 - 4600 B.P. :	Phase H: minor but distinct and apparently eustatic transgression.
4600 - 4300 B.P. :	Eustatic regression.
4300 - 4200 B.P. :	Minor but apparently eustatic transgression.
4200 - 3850 B.P. :	Eustatic regression.
3850 - 3600 B.P. :	Phase I: single, separate eustatic transgression.
3600 - 3500 B.P. :	Brief but apparently eustatic regression.
3500 - 3150 B.P. :	Phase J: single, separate eustatic transgression.
3150 - 2900 B.P. :	Eustatic regression.
2900 - 2700 B.P. :	Phase K: eustatic near stillstand (with slight rise ?)
2700 - 2450 B.P. :	Eustatic regression.
2450 - 2200 B.P. :	Phase L: slight, but probably eustatic, transgression.
2200 - 2100 B.P. :	Brief eustatic near stillstand (with slight fall ?)
2100 - 1900 B.P. :	Phase M: eustatic transgression.
1900 - 1800 B.P. :	Brief eustatic near stillstand (with slight fall ?)
1800 - 1650 B.P. :	Phase N: eustatic transgression.
1650 - 1250 B.P. :	Eustatic regression.
1250 - 1000 B.P. :	Phase O: evidence indecisive.

Figures 11.1 to 11.19 show the geographical distribution of the radiocarbon evidence discussed in Chapters 6 to 11, by 1° modules.



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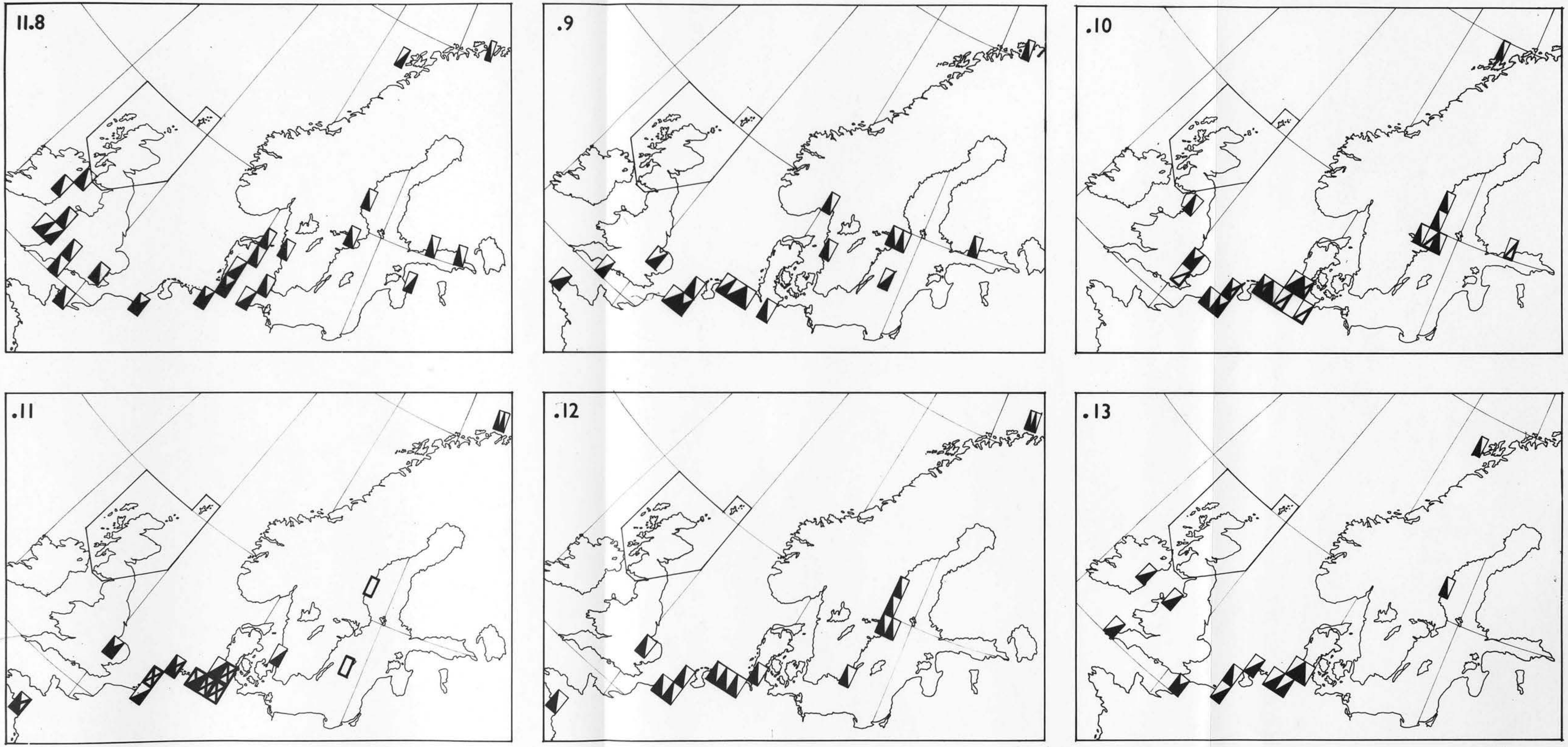
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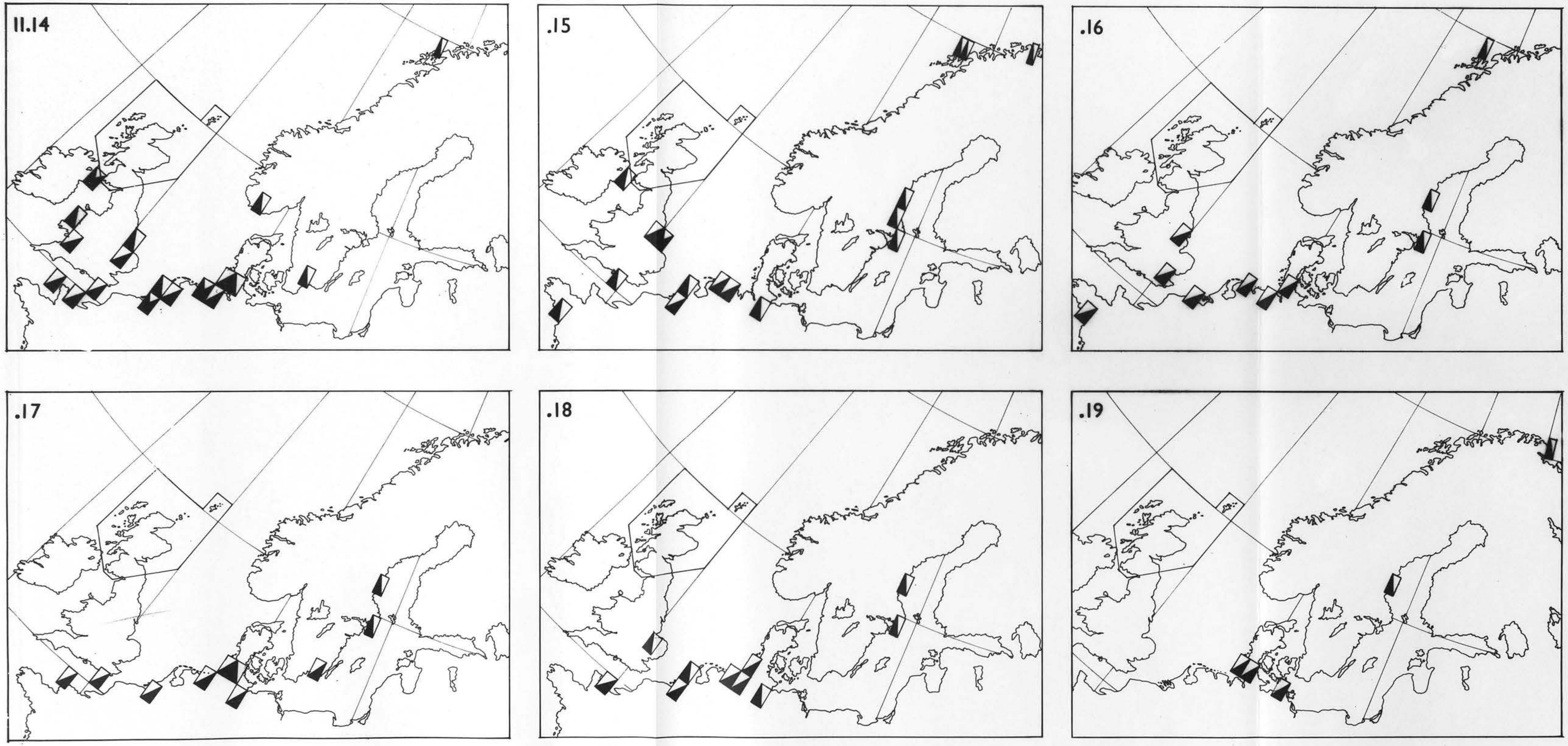

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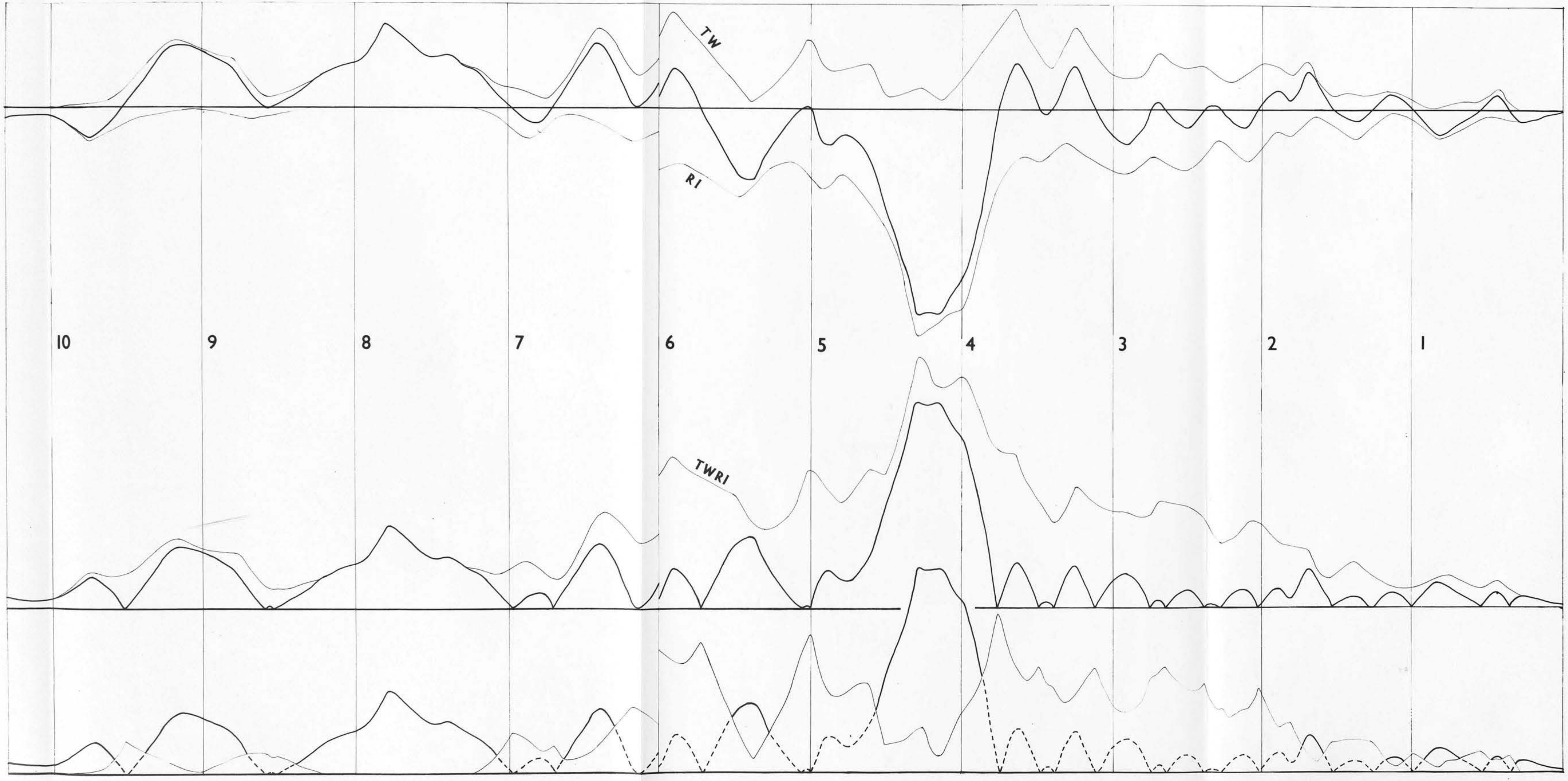
Figure 11.20 Graphs summarising the C^{14} evidence and illustrating variations in the "signal to noise" ratio through time.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